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VOL. III.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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PREFACE.

THE third Volume of this Miscellany being now completed, I have again the opportunity of making my acknowledgements to its friends and supporters, and presenting the respectable list of its Authors.

The authors of original Papers are John Bostock, M. D. Mr. Rob. Jameson; the Rev. James Wilson, A.M.; John Gough, Esq.; Mr. Ez. Walker; Rev. Joseph Priestley, L.L.D. F.R.S.; Rev. W. Pearson, P. R. I.; Dr. Thomas Young, F. R. S. and Professor at the Royal Institution; Mr. Peter Keir; Mr. John Dalton; Mr. Wm. Wilson; R. Chenevix, Esq. F. R. S.; Mr. John Cuthbertſon; Mr. John Murray; J. B.; W. N.; and Profeſſor Aldini.—Of foreign works; Hapel Lachenaie; Brongniart; J. B. Berard; Guyton; Gay Lussac; Ekeberg; B. Prevost.—And of English Memoirs abridged or extracted, Dr. Young; Mr. R. Phillips; Mr. J. Besant; Edw. Howard, Esq.; F.R.S.; Sir H. C. Englefield, Bart. F. R. S.; Mr. J. Dalton; Mr. H. Davy, Profeſſor at the Royal Institution; T. Wedgwood, Esq.; R. Chenevix, Esq. F. R. S. Mr. Wm. Henry.

Of the Engravings the Subjects are, 1. A powerful Blowpipe by Alcohol. 2. Mr. Wilson's two new Barometers. 3. An improved Water Wheel, by Mr. Besant. 4. Mr. R. Phillips's Tubes for driving Copper Bolts into Ships. 5. The Hydrometer of Atkins, for exhibiting Specific Gravities and

Strengths

Strengths of Spirits without Computation. 6. Mr. Read's very simple and cheap Pneumatic Apparatus. 7. The new moveable Crane erected by Mr. Keir at Ramsgate. 8. Haas's Blow-pipe Apparatus. 9. Figures by Mr. Jameson, to illustrate the Formation of Basalt. 10. Elements of the Galvanic Pile, by Dr. Bostock. 11. Distinguishers of Galvanism and Electricity. 12. Gay Lussac's Apparatus for measuring the Expansions of the Gases. 13. The Indian Hand Mill. 14. A Secret Lock, which can be opened only by the Proprietor. 15. Improvement of Woulfe's Apparatus, by Mr. Murray. 16. Dr. Young's Apparatus for exhibiting the colours of thin plates. 17. Figures by Prevost, for explaining the Phenomena of Dew.

Soho Square, Dec. 26, 1802.

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A JOURNAL OF NATURAL PHILOSOPHY, CHEMISTRY, AND THE ARTS.

SEPTEMBER, 1802.

ARTICLE I.

Deſcription of a cheap and ſimple Apparatus or Blow-pipe, in which the Flame of Oil or Tallow is impelled by the Vapour of Alcohol. From a Correſpondent.

To Mr. NICHOLSON.

SIR, *London, July* 9, 1802.

Inconveniences of the blow-pipe, and the ſubſtitutes commonly uſed.

USEFUL as the *blow-pipe* is for the profeſſed mineralogiſt and chemiſt, as well as for the amateur of the ſcience, it is, however, liable to the inconvenience of requiring one hand of the operator to be employed; beſides, that perſons of weak lungs, or not well accuſtomed to its uſe, find it laborious, even when they attempt to manage it according to the directions given by Bergman and Engſtrom; more eſpecially, when a continued ſtream of air is required for ſome time. For this reaſon, ſome have recourſe to a *bladder* filled with air, and force it out by preſſing the bladder between the arm and their body; others uſe a ſmall pair of *double bellows*, to be fixed upon the table: others again employ a receiver, or rather *gazometer*, in the form of Mr. Watt's hydraulic bellows. But all theſe means are not always ready at hand, or not eaſily portable to accom-

pany the travelling mineralogift, and in every cafe they require fome preparation. And even the ingenious contrivance of Mr. Haas, though it permits the free ufe of the operator's hands, is expenfive, and ftill requires the blowing through the mouth.

Very ufeful lamp exhibited by Profeffor Pictet.

When Profeffor Pictet of Geneva vifited London laft year, he brought along with him and exhibited to his friends an apparatus, which at the fame time that it is fimple and very portable, is the moft advantageous fubftitute for the blow-pipe, and removes all its inconveniences. An imitation of this apparatus was exhibited in the lecture-room of the Royal Inftitution, Albemarle-ftreet.

Defcription of two lamps for blowing the flame of oil by alcohol.

The drawing of this inftrument which I fubjoin, was copied from that communicated by a friend. A, plate 1, is a tin-box filled with common oil, ferving to feed the flame at F, thence to be directed on the fubftance under examination, and at the other flame, to heat the fpirit of wine contained in the copper or tin lamp B. The boiling fpirit being converted into fteam by the heat, rufhes through the bended tube C upon the flame of the wick at F. Laftly, D is a flider with loops and fcrews to raife or lower the fpirit lamp. The whole being very fimple and clear, any farther explanation would be needlefs.

Long ago ufed by Nollet.

I do not know whether Profeffor Pictet pretends to be the inventor of this apparatus; but certain it is, that Abbe Nollet more than fifty years ago employed fuch a contrivance, as may be feen in his *l'Art de faire Experiences*. Another application of the fpirit lamp for this purpofe I have read in the *Annales de Chimie*, but do not recollect in which volume.

There is no doubt but the communication of the lamp above defcribed will be acceptable to many of the readers of your excellent Journal; whence, if you agree with me in this opinion, be fo kind as to infert it.

I am, Sir,

Your humble fervant,

N. N.

ANNOTATION. W. N.

Powerful effect of a fmall eolipile.

The application of the elaftic and combuftible vapour of alcohol to this ufeful purpofe is fo obvious, that it can fcarcely be appropriated as an invention. The glafs-blowers have long fold a little implement, confifting of an egg-fhaped ball, having a ftem

a ſtem or handle, and a nozzle or tube. It is an eolipile, and when half filled with water or ſpirit, and heated over a candle, a ſtrong current either of vapour or of the denſe fluid will iſſue out accordingly as it is held. With the vapour of ſpirit thus blown through the large flame of a tallow lamp, I fuſed a halfpenny, and ſoftened the ſtopper of a bottle ſo as to run a wire through it; though I muſt confeſs not without apprehenſion of the thin brittle veſſel burſting, and covering the ſurrounding bodies with flame. The lamp B, which is the eſſential part, may be made cheap; and indeed the whole would coſt but a trifle. I took a ſmall lamp of that figure, $2\frac{1}{2}$ inches wide and $1\frac{1}{2}$ inch high (of the ſort called *agitable*, from the cap for the wick being made to ſcrew in and defend the oil), and in the place of the wick-pipe, I ſoldered a piece of a common blow-pipe. The lamp coſt eighteen pence, and the blow-pipe ſix-pence, and the combination anſwers perfectly well.

An halfpenny melted, and a glaſs ſtopper perforated.

The lamp apparatus is very cheap.

II.

Outline of the Hiſtory of Galvaniſm: with a Theory of the Action of the Galvanic Apparatus. By JOHN BOSTOCK, *M. D. From the Author.*

(*Concluded from Page* 304.)

COL. HALDANE found that the effects of the apparatus were ſuſpended when it was immerſed under water, and that this was alſo the caſe when it was placed under the vacuum of an air-pump. He conceived that the effects of the pile were not increaſed by its being connected with the conductor of an electrical machine, and he thought that the charging of the Leyden phial was retarded when the apparatus was placed between its outſide and inſide coatings. Upon the whole he concludes that its effects cannot be referred to electricity. Upon theſe experiments Mr. Nicholſon remarks, that in the caſe where the pile was connected with the conductor, the electric ſtream of the conductor might have been in a contrary direction to the electric ſtream of the pile. In the experiment where the pile was connected with the Leyden phial, it is certain that the phial could not be charged higher than the pile

Col. Haldane immerſed the pile in water. He diſcovered that its action is ſuſpended in vacuo.

Thinks it not electric.

Obſervations to the contrary.

itſelf,

itſelf, and from the effects of galvaniſm in general, it may be conjectured that it reſembles a large ſurface of an electric charged to a low intenſity.

Cruickſhank's ſimple apparatus for keeping the gaſes diſtinct.

Mr. Cruickſhank, in a ſecond communication, gives an account of ſome experiments which he performed with an interrupted wire of gold. By means of a bent glaſs tube he contrived to keep the gaſes obtained from each wire perfectly diſtinct. That from the ſilver end of the pile he found to be chiefly hidrogen, and that from the zinc end nearly pure oxigen. Both the gold wire and one which he uſed of platina were evidently tarniſhed on the zinc end, after being expoſed for ſome time to the influence. He draws the following general concluſions from his experiments. 1*ſt*, From the ſilver end, if it be immerſed in water, whatever be the nature of the wire, there is diſengaged a quantity of hidrogen gas mixed with a ſmall quantity of oxigen gas and ammonia. 2*d*, Where a metallic ſolution is uſed inſtead of water, the metal is reduced and is depoſited at the end of the wire. 3*d*, When the wire at the zinc end conſiſts of gold or platina, oxigen gas is diſengaged mixed with a little azote and nitrous acid; the quantity of gas is only about one-third of what is diſengaged at the ſilver end, ſo that the gaſes exiſt in the proportion requiſite to produce water. 4*th*, When the zinc wire is formed of an imperfect metal, it is partly oxidated, partly diſſolved, and a little oxigen only is diſengaged. Fluids which contain no oxigen Mr. Cruickſhank thinks are incapable of tranſmitting the influence, whilſt all thoſe which contain it appear capable of conducting the influence.

His general concluſions. 1. Silver end of the pile gives chiefly hidrogen. 2. Reduction of metallic ſolution at this end. 3. Zinc end gives chiefly oxigen. 4. Or produces oxidation.

Mr. Cruickſhank then proceeds to give an account of a new method of placing the metals horizontally in a kind of trough, which in ſome reſpects appears to be more convenient than the form of the apparatus originally invented by Volta.

His Invention of a new galvanic arrangement by a trough.

Mr. Davy obtains the oxigen from one portion of water, and the hidrogen from another unconnected portion.

Mr. Davy propoſed this curious ſubject of enquiry, whether the oxigen and hidrogen gaſes evolved by the interrupted wire when immerſed in water, can be procured from two portions of water kept diſtinct from each other? For this purpoſe he cauſed the wires to terminate in two glaſſes of water, and by means of his fingers eſtabliſhed a communication between the two glaſſes. The gaſes he found were produced as in the former caſe. When the water uſed was very carefully boiled, he found that the gaſes evolved were nearly pure, and in the pro-

portions

portions requisite to form water. Mr. Davy then wished to ascertain whether it was essential for the wires to be in contact with the metallic plates of the pile; the ends of the pile were therefore made to communicate with two glasses of water by pieces of muscular fibre, and the glasses were connected by a silver wire. The effects of the pile were now reversed. Hidrogen was disengaged by the wire at the zinc end, while the wire at the silver end became oxidated.

Reverse effect on a single intermediate wire.

Col. Haldane, in a second memoir, details a series of experiments made to determine the power of different combinations of metals. He found the pile acted more powerfully when immersed in a given quantity of oxigen gas, than when confined in the same bulk of atmospherical air; its action was entirely suspended in azote or in a vacuum. On this account he conjectures that the effects of galvanism depend upon a chemical operation, and that oxigen is attracted by the apparatus from the atmosphere.

Col. Haldane shews that the pile acts more strongly in oxigen, and not at all in azote or in vacuo.

Mr. Davy discovered that the influence of the pile was capable of being transmitted through charcoal, when an apparatus was constructed of this substance and silver. It decomposed water in the usual manner, the silver end giving out hidrogen with a little carbon in solution, while the zinc end evolved but little gas of any kind; this was probably owing to the absorption of the carbonic acid which would be formed at this end of the wire. He found that when the water between the pairs of metallic plates was perfectly pure, *i. e.* when it contained neither acid, salt, nor gas of any kind, the pile ceased to act. He conjectures that the energy of the pile is nearly in proportion to the rapidity with which the zinc becomes oxidated; and consequently the effects were found to be the most powerful when nitric acid was interposed between the metals. Mr. Davy appears in this instance to have made the first step towards the true theory of the action of Volta's pile: as might be inferred from this notion, he found that concentrated sulphuric acid interposed between the metals, acted less powerfully than diluted sulphuric acid, because this last has more power in oxidating zinc than when in its pure state. He also discovered that the pile can act in vacuo, provided a little acid be interposed between the plates. A pile composed of zinc and charcoal was found to possess great energy.

Davy's application of charcoal as the conductor.

His discoveries that pure water is ineffectual in the pile, and that its effects are proportioned to the oxidation.

He applies the acids,

and causes the pile to act in vacuo.

All

All theſe valuable diſcoveries and experiments are taken from original papers communicated to the 4th vol. of Nicholſon's Journal.

Anonym. that women are more ſuſceptible of galvaniſm than men.

In the 5th vol. of this work we meet with ſome curious obſervations detailed by an anonymous correſpondent at Edinburgh. He found that water which had been for ſome time ſubjected to the influence of the pile, continued for ſeveral days to emit gas, and to depoſit a mucous matter. He has obſerved that when both men and women form the galvanic circuit, the women appear to receive a larger ſhare of the ſhock than the men. Dr. Rutherford of Edinburgh conceives that vegetables are non-conductors of galvaniſm, and therefore in this particular that it differs from electricity.

Mr. Davy's pile with one metal only.

Mr. Davy afterwards diſcovered that a pile may be conſtructed with only one metal, provided proper fluids be applied to its ſurfaces. He employed the following ſeries: metal, nitrous acid, water, ſulphuret of potaſh, and then metal. This curious diſcovery, as Mr. Nicholſon judiciouſly obſerves, completely overthrows the hypotheſis of Volta.

Cruickſhank charges a jar with galvaniſm.

Mr. Nicholſon about this time was informed by Mr. Cruickſhank, that he had at length ſucceeded, by uſing proper precautions, in charging the Leyden phial by means of the galvanic pile.

Hitherto nearly all the wonderful diſcoveries that had been made by means of Volta's apparatus were confined to England, and were principally due to Mr. Nicholſon, Mr. Cruickſhank, Mr. William Henry, Mr. Davy, and Col. Haldane. The ſubject began now to attract the attention of foreigners, and we have an account of ſome curious experiments performed by Profeſſor Tromſdorff. He conſtructed a pile of zinc and copper, and fixed a piece of gold leaf to the zinc end of the wire; he found that it was eaſily conſumed, and that the ſame thing happened to other metallic leaves. This experiment was repeated by Fourcroy, and he adds this remarkable circumſtance, that ſix large zinc and ſilver plates gave only a ſlight ſhock, but conſumed the wire rapidly; whereas the ſame ſurface of metal arranged in the form of a greater number of ſmaller plates, afforded a more conſiderable ſhock, but would not burn the metal.

Tromſdorff burns gold leaf and other metals.

Remarkable diſcovery of Fourcroy, that the ſhock is greateſt when the repetition of plates is moſt numerous; the combuſtion greateſt the larger the ſurface.

About this time Mr. Cruickſhank obſerved, that when the wire of his trough apparatus terminated at the ſurface of the water,

Cruickſhank obſerves two dif-

water, the filver end emitted from the water a brufh of fire, at the fame time that the zinc end produced only a denfe fpark. This circumftance induced him to fuppofe that the filver was in the plus and the zinc in the minus ftate of electricity, which indeed agrees with Bennet's original experiments, though it differs from the account given by Mr. Nicholfon in his firft paper, and alfo from the experiments of Dr. Wollafton, Dr. Van Marum, and others, as we fhall fee below. ferent kinds of fpark.

Dr. Wollafton read a judicious paper to the Royal Society, which appeared in the Tranfactions for the year 1801. He remarks, as Mr. Davy had previoufly done, that the energy of the pile feems proportionate to the difpofition which one of the plates has to be oxidated or acted upon by the interpofed fluid. If a piece of filver and a piece of zinc be immerfed in diluted fulphuric acid, the zinc immediately begins to decompofe the water and to evolve hidrogen; the filver is not acted upon, but if we bring them into contact, the filver alfo begins to produce hidrogen. He therefore concludes, that in the folution of a metal by an acid, electricity is always difengaged. In order that the electrical machine fhould act ftrongly, it is neceffary that the amalgam upon the cufhion fhould be compofed of a metal which is eafily oxidated; the Doctor alfo found that an electrical machine plunged into carbonic acid gas is incapable of being excited. He concludes by the opinion which was fuggefted by Mr. Nicholfon, that electricity and galvanifm are the fame principle, but that in the latter it is generally met with in large quantity, but in a ftate of low intenfity. Dr. Wollafton coated two filver wires, leaving the ends only of them expofed; thefe wires were connected with the two conductors of an electrical machine, and the fpark was taken from one to the other through a folution of copper: he found that the end of the wire connected with the negative conductor was covered with reduced copper; no change had taken place in the other end. The negative conductor appears therefore, from thefe experiments, to correfpond with the filver end of the pile.

Dr. Wollafton. Oxidation difengages electricity, from the amalgam of the el. machine; which will not act in carbonic acid. He firft produced the galvanic effect on water by common electricity.

Mr. Davy's very curious difcovery, that a galvanic pile might be conftructed by one metal alone, provided its different furfaces were expofed to the action of different fluids, has been already announced: In addition to thefe facts the fame intelligent experimenter informs us, in the firft vol. of Nicholfon's Journal, N. S. that he has conftructed a galvanic apparatus which

Mr. Davy's pile with one fingle metal.

and of charcoal with no metal.

which contains no metal, and which confifts folely of pieces of charcoal, which have their different fides expofed to the action of different fluids.

Volta's account of his own theory.

In the fame volume there is an account of a paper written by Volta himfelf; but from the opinions which are advanced in it, we may conclude that he was unacquainted with many of the moft interefting circumftances which had been obferved in this country, refpecting the operations of the apparatus originally difcovered by himfelf.

Dr. Van Marum charges a large battety by galvanifm.

In the 40th volume of the Annales de Chimie, is a letter from Dr. Van Marum to Sig. Volta, containing an account of experiments performed in order to compare the effects of the galvanic pile with thofe of the great Teylerian machine at Haarlem. Both fingle jars and batteries were charged by means of the galvanic pile, and in all cafes they were charged to the fame degree of intenfity with that which the pile itfelf indicated to an electrometer. When the zinc was at the top of the pile, and communicated with the infide of the jar, the electricity of the infide was found to be pofitive, and when the pile was reverfed, it was negative. It was found that the fhocks given by the battery charged from the electrical machine, were not perceptibly different from thofe given by the battery when charged to an equal intenfity by the pile. By conftructing a pile of large plates of zinc and copper, he was enabled to fufe thick iron wires; he even fucceeded in fufing a wire of platina. He found that piles which confift of the fame number of plates, but of different diameters, give equal intenfities and equal fhocks; yet thofe with the larger plates he found to be confiderably more powerful in fufing metals.

Dr. Prieftley's experiments.

In the firft volume of Nicholfon's Journal, N. S. is a letter from Dr. Prieftley on the pile of Volta. It contains a number of interefting experiments performed by this venerable philofopher, principally with a view of eftablifhing his favourite hypothefis of phlogifton, to which he ftill adheres. Some of the facts which he notices had been previoufly obferved in England, though from his remote fituation he had not the opportunity of becoming acquainted with them. He interpofed four glaffes of water between the two ends of the pile; the glaffes were connected by means of filver wires, and the ufual operations went on at the legs of the wires; but it was found that when one of the portions of water had its furface covered with oil,

Galvanifm faid not to act on water under oil;

oil, the action was sufpended in the whole of the glaffes. This experiment has not, however, been found to anfwer in England. not confirmed.

Theory of the Action of the Galvanic Apparatus.

THE phenomena of the galvanic pile, which at firft view appear almoft incapable of being arranged into any fyftematic form, may, I think, be all eafily explained by admitting the truth of the three following *poftulates*. *1ft*, That the electric fluid is always liberated or generated when a metal or any oxidable fubftance is united to oxigen; *2dly*, That the electric fluid has a ftrong attraction for hidrogen; and, *3dly*, That when the electric fluid, in paffing along a chain of conductors, leaves an oxidable fubftance to be conveyed through water, it unites itfelf to hidrogen, from which it is again difengaged when it returns to the oxidable conductor.

Elementary pofitions of galvanifm. 1. Electricity is given out during oxidation. 2. Electricity ftrongly attracts hidrogen. 3. Electricity paffes (invifibly) through water, in combination with hidrogen.

The firft of thefe propofitions may be confidered as almoft proved by the experiments of Fabroni, Davy, and Wollafton; the fecond and third have not been directly eftablifhed by experiment, but will not appear improbable when it is feen in what a fimple and eafy manner they account for all the phenomena hitherto obferved. In endeavouring to explain the action of the pile, two diftinct fubjects of inquiry prefent themfelves: *1ft*, What are the operations carried on at the ends of the wires in the interrupted circuit, as difcovered by Mr. Nicholfon? and, *2dly*, What is the operation carried on in the body of the pile itfelf?

Nature of the proofs.

As the current of the electric fluid appears to pafs from the zinc or plus end of the apparatus to the filver end, we muft firft endeavour to afcertain the action which takes place at the zinc end of the wire. This in fact appears to be the difengagement of oxigen in a concentrated ftate, by which the wire itfelf, when oxidable, is corroded, but which, when the wire is formed of a perfect metal, is difengaged in the form of oxigen gas. This oxigen appears to be derived from the decompofition of the water in which the wire terminates, in confequence of the attraction which the electrical fluid poffeffes for hidrogen, and its incapacity of paffing through water without being united to this fubftance, according to the fecond and third poftulates.

Theory of what happens in Nicholfon's tube. El. paffes from the z. end and decompofes the water: the oxigen either appears or oxides the metal:—

The

The elect. and hidrogen pafs invifibly to the other point, where the firft is abforbed and the latter becomes gas; or if the fluid contain oxide, it reduces the metal and again forms water.

The electric fluid thus united to hidrogen, is carried to the other point of the wire, where, upon entering the oxidable conductor, it is difengaged in the form of hidrogenous gas, if water be the medium of communication; if a folution of a metallic oxide be employed, it unites with the oxide, and reduces it. The decompofition of water is therefore effected at the zinc point alone, though the different gafes which compofe it are difengaged at each of the points; and this procefs will continue even when the points terminate in two different portions of water, as was difcovered by Mr. Davy, provided that the glaffes are united by a conductor which is not oxidable.

Theory of the apparatus or pile.

This may be confidered as a general explanation of the firft fubject of inquiry; we muft next proceed to inveftigate the nature of the operation carried on in the body of the apparatus itfelf. In the conftruction of the pile there are two points which are effential to its action; *1ft*, That the electric fluid be difengaged; and, *2dly*, That it be confined and carried forward in one direction, fo as to be concentrated in the end of the apparatus.

The oxidation of the zinc developes elect. which decompofes the water, and is attracted by the hidrogen; and therefore paffes to the filver *through the fluid*, and not the reverfe. Its paffage to the next oxidating furface adds to the quantity of el. and increafes the oxidation; and this effect will increafe with the number of pairs.

The firft object is evidently attained by the oxidation of the zinc or other oxidable body employed. If both fides of the zinc were oxidated, the electric fluid would indeed be liberated, but it would be immediately difperfed, and its effects could not be obferved. As foon, however, as the electric matter is evolved, it is immediately attracted by the hidrogen, which is at the fame time neceffarily generated in the fluid which oxidates the metal, and it is by this means conveyed acrofs the water to the filver plate, when two metals are ufed; or, in other cafes, fimply to the oppofite furface of the oxidating fubftance. The electric fluid then enters the filver plate, and inftantly paffing on to the contiguous zinc plate, arrives at a fecond oxidating furface. The fame feries of events which have been defcribed is here repeated, except that the electric fluid being in fome degree accumulated in the metallic plate, is difengaged by the fecond oxidating furface in larger quantity, and in a more concentrated ftate, than before. By purfuing the fame train of operations, it is eafy to fee how the electric matter will continue to be accumulated in each fucceffive pair of plates, until, by fufficient repetition, it may be made to exift in the zinc end of the pile in any affigned degree of force.

Having

Having thus exhibited a general view of the hypothefis which has fuggefted itfelf to my mind, as affording a fimple explanation of the phenomena, without offering violence to any of the generally received opinions refpecting chemiftry or electricity, I fhall proceed to illuftrate its truth, by applying it to the explanation of fome of the moft remarkable properties of the pile. It is fufficiently obvious that upon thefe principles the action of the pile will continue as long as the furface of the zinc continues to be oxidated, and that, the fize of the pile remaining the fame, its energy will be proportionate to the rapidity of this operation. The real difference between the two ends of the apparatus will appear to be not fo much a difference in the abfolute quantity of electrical matter contained in them, as a difference in the direction of its current; becaufe exactly the fame quantity of fluid which paffes out of the zinc end muft be received at the filver end, when a communication is eftablifhed between them. It may be farther remarked, that when the pile is conftructed as directed by Volta, beginning with zinc, then filver, card and zinc, &c. though the zinc forms the bafis of the pile, yet as no change takes place in the electric fluid during its paffage from the filver to the zinc, this might with equal propriety have been called the filver end: in order, however, to avoid confufion, the ufual language has been adopted in this paper; the terms *difcharging* and *receiving* ends would be more appropriate.

Remarks and inferences from this theory.

It has excited a good deal of furprize, that, in the experiments of Mr. Davy and Mr. Cruickfhank, apparatufes, conftructed of various fhapes and of fuch very different materials, fhould be capable of producing the fame operations. We may in a few words enumerate the circumftances which are effential to its formation. *1ft.* An oxidable fubftance, one end or fide of which *only* is to be oxidated; *2d,* A fubftance from which the electric fluid generated during the oxidation can abftract hidrogen: thefe two fubftances muft be alternately arranged, either perpendicularly or horizontally; the firft fets the fluid at liberty, the fecond confines it and conducts it in the proper direction. When we admit the neceffity of the oxidation of the zinc, we immediately fee the reafon why the pile ceafes to act when perfectly pure water is interpofed between the plates, why the action is fufpended in vacuo and increafed by oxigen gas. The hypothefis advanced above fhews us how an

Confiderations on the apparatufes of Davy and Cruickfhank,

an apparatus may be conſtructed of one metal and charcoal, of one metal alone, or even of charcoal alone, provided a proper fluid be interpoſed to each ſurface. The experiment related by Mr. Davy, where, upon uniting the ends of the pile to two glaſſes of water by means of muſcular fibres, while the glaſſes themſelves were connected by a ſilver wire, the effects of the ends of the wire were reverſed, admits of a ſimple ſolution. The fluid muſt have been conducted along the fibres ſolely in conſequence of the moiſture attached to them; when it left the plus end of the pile it would therefore decompoſe part of this water, and paſs along in union with hidrogen to the ſilver wire, where the hidrogen would be diſengaged. The other end of the ſilver will then become oxidated, and the hidrogen abſorbed will be evolved at the minus end of the pile.

and Davy's extrication of the gaſes in ſeparate veſſels.

I have protracted this communication to ſo unuſual a length, that I ſhall only add one additional remark with reſpect to the fact firſt aſcertained by Fourcroy, that the power of the pile in giving ſhocks is not proportionate to its power in conſuming metals. A few alternations of large plates are more powerful in effecting the latter operation, while the ſame quantity of metal employed in conſtructing a higher pile of leſs diameter, affords a more powerful ſhock. It appears from every circumſtance, that the force of the ſhock depends almoſt entirely upon the concentration of the electric fluid; the ſame quantity which, when contained in a few jars highly charged, would be ſufficient to deſtroy animal life, if diſperſed over 100 jars would be ſcarcely perceptible. On the other hand, in the combuſtion of metals the abſolute quantity of electricity is of great importance; provided the current be rapid (which, from the experiments of Dr. Van Marum, appears to be the caſe with the pile of Volta in an amazing degree), and if the quantity of fluid be conſiderable, its ſtate of intenſity is of leſs moment *. The intelligent reader will at once perceive in what way the fact obſerved by Fourcroy admits of explanation from this train of reaſoning.

Fourcroy and Thenard's experiment that the power of giving ſhocks and burning wire depend on the mode of building the pile;

referred to electrical phenomena exhibiting the ſame reſult with coated jars.

* Cuthbertſon firſt obſerved in a letter to me (1793) the ſtriking fact, that the combuſtion of equal lengths of the ſame wire is made by equal quantities of electricity, whether the intenſity be greater or leſs (within certain extended limits). See Account of Experiments. Philoſ. Journal, quarto, II. 215. Auguſt, 1798.—N.

III.

On the ſuppoſed Exiſtence of Mechanical Depoſits and Petrefactions in the Primitive Mountains, and an Account of Petrefactions which have been diſcovered in the neweſt Flötz Trapp formation. By Mr. ROBERT JAMESON. *Communicated by the Author.*

The primitive rocks precipitated from a great depth of water which covered the globe.

OUR globe, according to the Wernerian geognoſia, even during the depoſition of the newer primitive ſtrata, appears to have been covered to a great height with water, as is evinced by the want of all mechanical depoſit *. After the precipitation of theſe great rock formations, the level of the water became ſo low, as to allow it to act mechanically upon the ſubjacent rocks; this occaſioned the firſt mechanical depoſition, which diſcovers itſelf in the tranſition rocks (Ubergangsgebürge). Nearly at the ſame time organization commenced, as it is in the tranſition rocks we find the firſt traces of organic remains: theſe are generally zoophytes and ſea plants, a fact which goes deep not only into geology, but natural hiſtory.

The tranſition rocks when the water was lower: at which time organization began.

Inſtances of mechanical depoſits in primary ſtrata: offered by Profeſſor Playfair.

Profeſſor Playfair, in his illuſtrations of the Huttonian theory, mentions ſeveral inſtances of mechanical depoſits and petrefactions which have been diſcovered among the primary ſtrata, and from theſe he concludes, that no ſuch ſeries of ſtrata as the tranſition exiſt. I ſhall now examine the ſtatements he has given; and firſt reſpecting the occurrence of petrefactions in primary mountains.

Shells in primitive limeſtone, in the vicinity of Plymouth, &c.

At page 164 he obſerves, " Another ſpot, affording inſtances of ſhells in primitive limeſtone, is in Devonſhire, on the ſea ſhore, on the eaſt ſide of Plymouth dock, oppoſite to Stonehouſe, I found a ſpecimen of ſhiſtoſe micaceous limeſtone, containing a ſhell of the bivalve kind; it was ſtruck off from the ſolid rock, and cannot poſſibly be conſidered as an adventitious foſſil. Now, no rocks can be more decidedly primary than thoſe about Plymouth; they conſiſt of calcareous ſtrata, in the form either of marble or micaceous limeſtone, alternating with ſhiſtus of the ſame kind, which prevails through Cornwall to the weſt, and extends eaſtward into Dartmoor, and on the ſea coaſt as far as Berry Head. Theſe all inter-

* The exception to this in the ſienite formation I explained in a former paper.

ſect

fect the horizontal plane in a line from eaſt to weſt nearly; they are very erect, thoſe at Plymouth being elevated to the north." That petrefactions exiſt in the limeſtone at Plymouth is evident; but that theſe ſtrata are primitive, ſtill remain to be proved. The character given of the limeſtone does not exclude it from the tranſition ſtrata; but of the ſhiſtus we cannot judge, as neither its oryctognoſtical, or geognoſtical characters are given. The other inſtances which are alluded to, are liable to the ſame objection. I cannot therefore agree with Profeſſor Playfair in believing "Though, therefore, the remains of marine animals are not frequent among the primary rocks, they are not excluded from them; and hence the exiſtence of ſhell fiſh and zoophytes, is clearly proved to be anterior to the formation even of thoſe parts of the preſent land which are juſtly accounted the moſt ancient."

But it is not proved that thoſe ſtrata are primitive.

Whence the concluſion of the Profeſſor is not agreed to.

Profeſſor Playfair agrees with Dr. Hutton in affirming, that vegetable matters occur in the primitive ſtrata: I do not find it neceſſary to enter into an examination of what they have ſaid upon this ſubject, as they have evidently confounded a geognoſtical with an oryctognoſtical inveſtigation.

The poſition that vegetable matters are found in primitive ſtrata need not be examined.

I ſhall now examine the proofs which Profeſſor Playfair has brought to eſtabliſh the exiſtence of mechanical depoſits in primitive ſtrata.

Prof. Playfair's proofs of mechanical depoſits in primitive ſtrata examined.

The firſt we meet with is from Sauſſure. Profeſſor Playfair remarks, "St. Gothard is a central point, in one of the greateſt tracts of primary mountains on the face of the earth, yet arenaceous ſtrata are found in its vicinity. Between Ayrolo and the Hoſpice of St. Gothard, Sauſſure found a rock, compoſed of an arenaceous or granular paſte, including in it hornblende and garnets. He is ſomewhat unwilling to give the name gres to this ſtone, which Mr. Beſſon has done; but he nevertheleſs deſcribes it as having a granulated ſtructure."

Mount St. Gothard a central primitive mountain, has arenaceous ſtrata in its vicinity.

The rock of Ayrolo is primitive, and is either gneiſs or mica ſlate. Garnets are ſeldom found in gneiſs, but are characteriſtic for mica ſlate; the geognoſt, therefore, would not heſitate to conſider the rock here mentioned as belonging to mica ſlate.

Obſervation. The rock here mentioned was gneiſs or mica ſlate.

Profeſſor Playfair continues, "Among the moſt indurated rocks that compoſe the mountains of this iſland, many are arenaceous. Thus, on the weſtern coaſt of Scotland, the great body of high and rugged mountains on the ſhores of Araſaig, &c.

Inſtances by P. Playfair of indurated arenaceous granite.

&c. from Ardnamurchan to Glenelg, confifts, in a great meafure, of a granitic fandftone, in vertical beds. This ftone fometimes occupies great tracts; at other times, it is alternated with the micaceous, and other varieties of primary fhiftus; it occurs, likewife, in feveral of the iflands, and is a foffil which we hardly find defcribed or named by writers on mineralogy."

The granitic fandftone of Glen-elg is moft certainly gneifs *, and a variety which is not uncommon; and I may venture to fay, that the ftrata in Arafaig, &c. are of the fame nature.

Obf. This granitic fandftone is gneifs.

Profeffor Playfair concludes with ftating the following examples, as a further confirmation of his opinion—" Much alfo of a highly indurated, but granulated quartz, is found in feveral places in Scotland, in beds of ftrata, alternated with the common fhiftus of the mountain. Remarkable inftances of this may be feen on the north fide of the ferry of Balachulifh, and again on the fea fhore at Cullen. At the latter, the ftrata are remarkably regular, alternating with different fpecies of fhiftus. At the former, the quartz is fo pure, that the ftone has been miftaken for marble.

Other examples of Pr. Playfair. Granulated quartz in beds alternating with the fhiftus.

" Thefe examples are perhaps fufficient; but I muft add, that in the micaceous and talcofe fchifti themfelves, thin layers of fand are often found interpofed between the layers of mica or talc. I have feen a fpecimen from the fummit of one of the higheft of the Grampian mountains, where the thin plates, of a talcy or afbeftine fubftance, are feparated by layers of a very fine quartzy fand, not much confolidated. The mountain from which it was brought, confifts of vertical ftrata, much interfected by quartz veins. It is impoffible to doubt, in this inftance, that the thin plates of the one fubftance, and the fmall grains of the other, were depofited together at the bottom of the fea, and that they were alike produced from the degradation of rocks more ancient than any which now exift."

Whence he concludes that they were depofited (mechanically) at the fame time.

I am furprized Profeffor Playfair fhould adduce granular quartz as a proof of mechanical depofition, as it has no more claim to fuch a character than granite. The following obfervations will render this evident; granular quartz differs from mica flate, in the abfence of the flaty fracture, and mica; we have,

Obf. Granular quartz is not a mechanical, but a chemical depofition. For this differs from mica flate in the abfence of flaty fracture; but the feries

* Mineralogy of the Scottifh ifles, vol. ii. p. 160.

between each are gradual.
Mica ſlate differs from gneiſs in wanting felſpar; but here alſo the tranſition is equally gradual.
And gneiſs differs from granite by its ſlaty fracture, which it as gradually loſes.
The gradation from granular quartz to granite being perfect, the one is no more a mechanical depoſit than the other.

have, however, a ſeries from the moſt complete granular quartz to the moſt perfect mica ſlate. Again, mica ſlate, which differs from gneiſs, in wanting felſpar, is to be obſerved in all the intermediate ſtages until it paſſes into complete gneiſs; the gneiſs, which is principally diſtinguiſhed from granite by its ſlaty fracture, gradually loſes this fracture, and at length is not to be diſtinguiſhed from granite. Thus we have a complete gradation from the pureſt granular quartz to granite, and not only in hand ſpecimens, but in the mountains themſelves. It therefore follows, that if granular quartz is a mechanical depoſit, ſo is granite, a poſition which I believe the Huttonian ſyſtem would not allow.

Looſe texture is no proof of mechanical depoſition;
and layers of ſand are found in veins which in the volcanic theory are ſaid to bear every mark of complete fuſion.

That the granular quartz ſhould occur ſometimes of a very looſe texture; nay, even as Profeſſor Playfair remarks ſandy, is not ſurprizing, for in granite and baſalt we have ſimilar appearances. Even in veins, which to uſe Profeſſor Playfair's own words, bear all the marks of complete fuſion, layers of granular quartz, from the moſt compact to the looſeneſs of ſand have been obſerved. The great ſand veins in the Hartz afford remarkable inſtances of this; alſo, as I have more lately diſcovered, the lead veins at Wanlock-head in Lancaſhire.

Account of Organic remains which have been diſcovered in the neweſt Flötz Trapp formation.

General obſervations.

Many different accounts had been given of the geognoſtic relations of the rocks of this formation, before they engaged the attention of Werner, the great founder of *true geognoſia*. After having made the remarkable diſcovery upon the hill of Scheibenberg, he extended his inquiries to all the baſalt hills in Germany, and found in every quarter correſponding appearances. This confirmed more completely the concluſions he had then drawn, and intirely overthrew the volcanic ſyſtem. He however did not ſtop here; his after obſervations diſcloſed a connection among theſe appearances, which at firſt he was not probably aware of; they placed the Neptunian ſyſtem beyond the reach of attack, and completely annihilated a hoſt of hypotheſes. He proved,

Reſults. 1. That this is the neweſt of the great rocks.
2. That all the now unconnec-

1. That this is the neweſt of all the great rock formations, of which the cruſt of the earth is compoſed.

2. That all the apparently unconnected hills and maſſes of this formation, have formerly ſtood in connection with each other.

3. That

3. That it exifts in all quarters of the globe.

Thefe facts lead him to the great difcovery, that this formation at one time, extended as a cover around the whole earth. From thefe obfervations it is evident, that we may expect to meet with veftiges of many of the organic and unorganic matter which at that time exifted upon the cruft of the earth, in the rocks of this formation. Accordingly, the inveftigations of geognofts have difcovered organic remains in every rock of this formation; the following are given as inftances:

ted maffes of this formation were once connected. 3. That it exifts every where. 4. That it formerly covered the whole earth, and muft contain organic remains.

Organic remains in Greenftone, Bafalt, Wacke, and Trapp Buccin.

QUADRUPEDS.

Organic remains in greenftone, bafalt, &c. found; viz. of quadrupeds.

1. Werner in his geognoftic lectures, informs us that the wacken of Kalten-nordheim is fometimes found to contain deers horns.

2. The Abbe Fortis difcovered the head of an unknown animal in a foft wacken, in the valley of Ronca in the Veronefe. *Vide Befchrebbung des Thales Ronca, f.* 96.

3. Sauffure obferved bones of quadrupeds in the wacken of the catacombs of Rome.

Lettre a Mr. le Chev. Hamilton, J. de Phyfique. Tom. VII.

SHELLS AND ZOOPHYTES.

Of fhells and zoophytes.

1. Dr. Richardfon found fhells in rocks of the Trapp formation at Ballycaftle in Ireland.—*Kirwan's Geol. Effays.*

2. Mr. Von Buch informs us, that in the county of Landeck, he obferved a bed of wacken, which contained befides pebbles of chalcedony, turbinites in a ftate of complete prefervation. *Verfuch einer mineralogiften Vefchreibung von Landeck, von Leopold von Buch. f.* 35.

3. Abbé Fortis obferved numerous petrefactions of fhells in Wacken, and Trapp Breccia, alfo a few in Bafalt, in the formation of the valley of Ronca.

4. Berolding found a cornu ammonis, which ftill retained its mother of pearl luftre, in the bafalt of Torez. In the bafalt of Thurgau, near the Boden lake, he obferved gryphites, ammonites, and gloffopetræ, *Chem. Annal.* 1794, p. 103.

5. In the Wernerian collection of petrefactions I faw fpecimens of greenftone, containing petrefactions of fhells.

Of vegetables.

VEGETABLE REMAINS.

1. Werner obferved great trees, with branches, leaves, and fruit, in the wacken, at Joachimfthal.

2. Friefleben defcribes the impreffion of a plant in the Kawfower Berg, near to Podfedlitz.

3. In the iflands of Banna and Skye I obferved pieces of wood in Trapp Breccia, and Mineralogy of the Scottifh ifles, vol. ii. p. 58—75.

Organic remains in flaty clay, limeftone, and fandftone, of this formation.

Organic remains found in flaty clay, limeftone, and fandftone, which belongs to the Flötz Trapp formation.

SHELLS.

Shells.

1. Abbé Fortis, in his account of the valley of Ronca, informs us, that limeftone and flaty clay often alternate with bafalt, wacken, and trapp breccia. The limeftone and flaty clay contains numerous petrefactions of fhells, which are of the fame kinds with thofe found in the bafalt, wacken, and trapp breccia.

2. In the ifland of Eigg, where there appears to be a fimilar formation with that of Ronca, I obferved that the limeftone, flaty clay, and fandftone, contained numerous petrefactions of fhells. Mineralogy of the Scottifh ifles, vol. ii. p. .

Of vegetables.

VEGETABLE REMAINS.

1. At the northern extremity of the ifland of Skye, where bafalt alternates with limeftone and flaty clay, I obferved pieces of carbonated wood in the limeftone. Mineralogy of the Scottifh ifles, vol. ii. p. 80 *.

2. In the flinty fandftone, which ufually accompanies this formation, I have obferved branches of fhrubs; vegetable matters that occur inveloped in rocks are generally, either carbonated or bituminated, here however they are not altered.

R. JAMESON,

Sheriff Bræ, Leith.

* Mr. Kirwan, in the firft volume of his Syftem of Mineralogy, has given us an excellent account of the different opinions refpecting the formation of bafalt; and has adduced many arguments that fhew the fallacy of the Volcanic and Plutonic hypothefis.

POSTSCRIPT.

POSTSCRIPT.

Since writing the inclofed, I have examined one of thofe appearances, which are confidered by Dr. Hutton and Profeffor Playfair, as demonftrating the exiftence of petrefactions in primitive mountains. Dr. Hutton, at p. 334 of his Theory of the Earth, remarks, " I have already obferved, that one fingle example of a fhell, or of its print, in a fchiftus, or in a ftone ftratified among thofe vertical or erected maffes, fuffices to prove the origin of thefe bodies to have been, what I had maintained them to be, water formed ftrata created from the bottom of the fea, like every other confolidated ftratum of the earth. But now, I think, I may affirm that there is not, or rarely, any confiderable extent of country of the primary kind, in which fome mark of this origin will not be found, upon careful examination; and now I will give my reafon for this affertion. I have been examining the fouth alpine country of Scotland occafionally, for forty years back, and I could not find any mark of an organized body in the fchiftus of thofe mountains. It is true, that I knew of only one place where limeftone is found among the ftrata: this is upon Tweedfide near the Crook. This quarry I had carefully examined long ago, but could find no mark of any organized body in it. I fuppofe they are now working fome other of the vertical ftrata near to thofe which I had examined: for, in the fummer of 1792, I received a letter from Sir James Hall, which I fhall now tranfcribe. It is dated Moffat, June 2, 1792.

Dr. Hutton's ftatement of facts to fhew the mechanical origin of primitive countries from the exiftence of organic remains.

" As I was riding yefterday between Noble Houfe and the Crook, on the road to this place, I fell in with a quarry of alpine limeftone; it confifts of four or five ftrata, about three feet thick, one of them fingle, and the reft contiguous; they all ftand between the ftrata of flate and fchift, that are at that place nearly vertical. In the neighbourhood, a flate quarry is worked of pure blue flate; feveral of the ftrata of flate near the limeftone, are filled with fragments of limeftone fcattered about like the fragments of fchift in the fandftone, in the neighbourhood of the junction on our coaft. Among the maffes of limeftone lately broken off for ufe, and having the fracture frefh, I found the forms of cockles quite diftinct, and in great abundance. I fend you three pieces of this kind," &c.

Sir James Hall's account of organic remains in a limeftone ftated to be primitive.

It may perhaps be alledged, that thoſe mountains of Cumberland and Tweedale are not the primary mountains, but compoſed of the ſecondary ſchiſtus, which is every where known to contain theſe objects belonging to a former earth. Naturaliſts who have not an opportunity of convincing themſelves by their proper examination, muſt judge with regard to that geological fact by the deſcription of others. Now it is moſt fortunate for natural hiſtory, that it has been in this range of mountains that we have diſcovered thoſe marks of a marine origin; for, I ſhall afterwards have occaſion to give the cleareſt light into this ſubject, from obſervations made in other parts of thoſe ſame mountains of ſchiſt, by which it will be proved that they are primary ſtrata; and thus no manner of doubt will remain in the minds of naturaliſts, who might otherwiſe ſuſpect that we were deceiving ourſelves, by miſtaking the ſecondary for the primitive ſchiſtus.

Remark that the limeſtone is not primitive,

Dr. Hutton's account of the mountains in the ſouth of Scotland is confuſed and unſcientifical, and hardly comes within the pale of *true geognoſia.* It is not my intention, at preſent, to enter into an examination of his obſervations; the object of this poſtſcript is to ſhew, that the limeſtone between Noble Houſe and the Crook Inn does not belong to the primitive mountains. The beds of limeſtone mentioned by Sir James Hall, I obſerved lying between ſtrata of tranſition ſlate, and this ſlate alternating with ſtrata of grey wacke; conſequently the whole belongs to the tranſition claſs of rocks.

for it lies between ſtrata of tranſition ſlate; alternating with ſtrata of grey wacke.

Deſcription of the ſeveral rocks.

The limeſtone has a blueiſh grey colour, fracture is foliated, the diſtinct concretions are from coarſe to fine grained, and it is hardly tranſlucid on the edges. It is often traverſed by veins of calcareous ſpar, and ſometimes it contains thin beds of flinty ſlate (Kieſel Schiefer of Werner). The tranſition ſlate has a blueiſh or ſmoke grey colour, has generally leſs luſtre than the primitive ſlate, and contains much interſperſed mica. I obſerved it in all the ſtages from nearly pure ſlate to grey wacke. The grey wacke is compoſed of fragments of tranſition ſlate, flinty ſlate, and quartz, connected by a baſis of tranſition ſlate. It is frequently traverſed by veins of quartz, and is to be obſerved where the fragments are hardly diſtinguiſhable from their ſize; it has much the appearance of a breccia. I ſhall take another opportunity of ſending you drawings of the different kinds of petrefactions that occur in the limeſtone.

IV. *A*

IV.

A Method of increasing the Senfibility of the Barometer, ad libitum. By the Rev. JAMES WILSON, *A. M. Communicated by the Author.*

THOUGH there have been many contrivances for making barometers with indefinite fcales, they all feem to labour under fome difficulty either in their conftruction or ufe.

> Contrivances to enlarge the fcale of the barometer

The evaporation of water in that of Des Cartes, the fluggifh motion of the mercury in the horizontal and diagonal ones, the friction in Dr. Hooke's, and the unfteadinefs of floating bodies in Rownings, have rendered thefe ingenious contrivances fcarce preferable to the common vertical one.

> by water; by horizontal or inclined columns, by mechanifm and a float, &c. have not proved ufeful.

None of thefe defects, I believe, will attach to that which I propofe. It is as follows: (See Fig. 1. Plate II.) A B, a tube differing in no refpect from that of the common barometer, but that it is wider and longer, fo that a cylindrical rod, *q r*, may float freely on the mercury, and that the lower end is connected to a fmall bent tube B C D, inftead of being immerfed in a ciftern. To the lower end of the floating rod is faftened a hair, or fine iron wire, (what I ufed was a piece of the Indian weed ufed by anglers) which is brought through the mercury, and out at D, fo that the rod may be either drawn down, or fuffered to afcend at pleafure.

> Defcription of a new barometer. A metallic rod floats in the mercury of a fyphon barometer.

Upon the tube C D is a mark N, to which by moving the rod we can at any time bring the furface of the mercury. For by drawing it down we force up mercury into each tube, and on the other hand, by fuffering it to afcend, we fuffer the mercury to defcend in each.

The furface of the mercury then being brought to this mark, and the rod being faftened, by tying the hair to the pin E, if on infpecting it afterwards, we find that the furface of the mercury has moved from N, we are fure there has been fome variation in the weight of the atmofphere, viz. if it has fallen, we know that this weight has increafed, and *vice verfâ*.

> and is drawn down into the mercury till the internal rife caufes the lower furface to ftand at a fixed mark.

The variations either at M or N are in a given ratio to the variations in the length M N of the fuftained column. For the rife or fall at M, is to the fimultaneous fall or rife at N, in the inverfe ratio of the bafes, or horizontal fections of the mercury

> The variations at the inner or outer furface are proportioned to thofe of the whole column.

mercury at M and N; namely, as the area of the horizonta ſection of the tube D C is to the difference of the areas of the horizontal ſections of the tube A B, and of the rod. That is, if D, d, and r be the diameter of the tubes A B, D C, and rod, as $d^2 : \overline{D^2 - r^2}$, which is a given ratio, and conſequently each quantity has to the ſum of both, which is the variation in the length M N a given ratio, *e. gr.* d^2 (or $D^2 - r^2$) : $D^2 + d^2 - r^2$.

The variations either at M or N would therefore, if we could meaſure them with ſufficient accuracy, expreſs truly the variation in the weight of the atmoſphere.

and ſo are thoſe in the riſe or fall of the rod; if it be in every caſe adjuſted to the mark. And this meaſure may be enlarged at will. Proof.

But if inſtead of attempting this, we move the rod by means of the hair until the mercury return to N, by the length of rod moved either into the mercury, or out of it, we have another meaſure, and one as long as we pleaſe, of the variations in the weight of the atmoſphere.

For, ſuppoſing the mercury to have fallen at N, any ſpace N x, and riſen at M, a ſpace My, for the adjuſtment we muſt draw down the rod until it has diſplaced as much mercury as will fill the ſpace N x, and alſo a ſpace $y\,x$ in the other tube of preciſely the ſame bulk, through which it muſt riſe at the ſame time. The length of rod drawn down for each of theſe purpoſes will have a given ratio to the ſpace N x, and conſequently their ſum added to My will have a given ratio to it: and thence, as N x is a meaſure of the variation of the atmoſphere, this portion of rod will alſo be a meaſure: and manifeſtly it may be increaſed at pleaſure by leſſening the thickneſs of the rod.

This appears as follows. The proportion of the riſe in one tube to the ſimultaneous fall in the other upon any change of the weight of the atmoſphere, was above determined to be as $\frac{1}{D^2 - r^2} : \frac{1}{d^2}$. Let q be the *quantity* of mercury transferred from one tube to the other, the heights My and N x will be $= \frac{1}{D^2 - r^2}$ and $\frac{q}{d^2}$ and the ſum of theſe is the variation which would take place in De Luc's barometer.

The variation M$y = \frac{q}{D^2 - r^2}$ is ſeen on the rod without moving it; upon moving it there is added to this, firſt, $\frac{q}{r^2}$ to

to occaſion the aſcent $N x$ (the bulk of rod drawn down for that purpoſe being = to the bulk of q, and thence its altitude $= \frac{q}{r^2}$) and ſecondly, $\frac{q D^2 - q r^2}{d^2 r^2}$ to occaſion the equal ſimultaneous aſcent $y x$ (ſince the bulk of rod drawn for that purpoſe is greater than the bulk q in the ratio $D^2 - r^2 : d^2$, and therefore equal to $\frac{q D^2 - q r^2}{d^2}$ and its altitude equal to $\frac{q D^2 - q r^2}{d^2 r^2}$).

The ſum of theſe three $\frac{q}{D^2 r^2} + \frac{q}{r^2} + \frac{q D^2 - q r^2}{d^2 r^2}$ is the whole variation ſeen on the rod. This is to $\frac{q}{D^2 - r^2} + \frac{q}{d^2}$, the variation on De Luc's ſcale, (by bringing them to a common denominator) as $D^4 - 2 D^2 r^2 + D^2 d^2 + r^4 : D^2 r^2 + d^2 r^2 - r^4$, which is a given ratio; and therefore the rod affords an accurate meaſure.

Let s and 1 be the rod ſcale, and the common ſcale $s = \frac{D^4 - 2 D^2 r^2 + D^2 d^2 + r^4}{D^2 r^2 + d^2 r^2 - r^4}$, which by performing the diviſion gives the infinite ſeries $\frac{D^2}{r^2} - 1 + \frac{d^2}{D^2} - \frac{d^4}{D^4} +$ &c. $\frac{D^2}{r^2}$ is the principal term of this quote; for the ſum of the infinite ſeries commencing with -1 is $\frac{-D^2 + r^2}{D^2 + d^2 - r^2}$ which is neceſſarily leſs than a negative unit; and if d be ſuppoſed to vary, decreaſes ſtill as it increaſes.

The ſcale therefore being increaſed in ſo trifling a degree by the increaſe of d, may be ſaid to be as $\frac{D^2}{r^2}$.

The narrower the tube D C is I ſhould ſuppoſe the better; ſince though the ſcale is diminiſhed in a ſmall degree, yet the variation $N x$ is rendered more ſenſible, which is a point of great importance.

Injury from agitation is thus alſo in ſome degree prevented.

The ſcale of this barometer beſt obtained from obſervation.

Though we can thus from D, d, and r calculate the ſcale, yet as we can ſcarcely meaſure them with ſufficient accuracy, the

the beſt way is to determine it by obſervation, viz. by finding how much of the rod expreſſes ſome larger variation in the common barometer, and then taking out the rod and graduating, &c. or rather by taking the mean of a great number of obſervations.

Should this be found on a ſufficient trial to anſwer the purpoſe, I can point out a method of making it ſerve alſo as a thermometer.

This barometer was ſuggeſted by reflection on another conſtruction.

Before I conclude, I ſhall juſt mention the idea which led me to this conſtruction, which though the greateſt increaſe of ſcale it gives is 14 : 1, may poſſibly in the end be found to anſwer better.

Water poured on the expoſed ſurface of a ſyphon mercurial barometer gives a barometer which may be made to ſhew variations 14 times as long as the mercurial change.

It ſtruck me that if we were to pour in water into a long tube D F, connected with the tube of the common barometer as in Fig. 2, or to take out water ſo as at any time to bring the mercury to the fixed point N, the altitude of water either poured in or taken out would give us a meaſure for the variations of the barometer fourteen times larger than in the common one, ſince water is fourteen times lighter than mercury. This is evident from the common hydroſtatical principles, as the altitude of water poured in is to be a counterpoiſe to the ſum of the fall of mercury in A B, and riſe in C D, and *e contra,* the altitude of water taken out is to be a counterpoiſe to the ſum of the riſe of mercury in A B, and fall in C D.

If the tube D F therefore were graduated, it would give a ſcale 42 inches long, and the riſe of water would indicate diminution in the weight of the atmoſphere and *v. v.* thence the ſcale to be inverted. To facilitate the pouring out and in of water, let there be a bucket H moveable up and down by rackwork, or otherwiſe, from D to F; to this let there be attached the ſyphon S.

By raiſing the bucket until the level of the water in it be higher than the level of that in the tube, we pour in water at pleaſure, and *e contra,* by bringing down the bucket we take out water at pleaſure.

This inſtrument will have all the advantages of a water barometer without its unmanageable length; nor do I ſee any objection to it, except perhaps that its appendages are too bulky *.

V. *A Summary*

* The principal objection to the ingenious contrivances in the preſent communication are, that the reſults originally depend on the

V.

A Summary of the most useful Parts of Hydraulics, chiefly extracted and abridged from Eytelwein's Handbuch der Mechanik und der Hydraulik. Berlin, 1801. *By* THOMAS YOUNG, *M. D. F. R. S.**

THE theory of hydraulics has never been carried to a very high degree of perfection upon mathematical foundations alone; nor has it hitherto, even with the affistance of experiment, been rendered of much practical utility. Newton began the inveftigation of the motions of fluids: Daniel Bernoulli added to Newton's propofitions much valuable matter, both from calculation and from experiment; D'Alembert and many later authors have exercifed their analytical talents in inquiries of a fimilar nature. But another and a more practicable mode of attaining hydraulic knowledge has been attempted by a diftinct clafs of inveftigators, at the head of whom ftands the Chevalier de Buat. Thefe have begun from experiment alone, and have laborioufly deduced from very ample obfervations of the actual refults of various particular cafes, the general laws by which the phenomena appear to be regulated, or at leaft the formulas by which the effect of new combinations may be predicted. But it muft be confeffed that thefe formulas, however accurate, are too intricate to be retained in the memory, or to be very eafily applied to calculations from particular data.

State of hydraulic theory, and difficulties of applying it to practice.

Mr. Eytelwein, a gentleman already known to the public by his tranflation of Buat's work into German, with important additions of his own, and honoured with feveral employments and titles relative to the public architecture of the Pruffian dominions, has collected into this compendium of mechanics and hydraulics, the principal facts that have been afcertained, as well by his own experiments, as by thofe of former authors,

Mr. Eytelwein's valuable compendium.

the accuracy of adjuftment at N, in which the errors are precifely the fame as in the common barometer. The double barometer affords an inftance of a lighter fluid floating on a larger; but with a fomewhat fhorter fcale than that of fig. 2.—N.

* This excellent memoir is copied from the Journals of the Royal Inftitution.

efpecially

eſpecially ſuch as are the moſt capable of practical application; and he appears to have done this in ſo judicious a manner, as to make his book a moſt valuable abſtract of every thing that can be deduced from theory reſpecting natural and artificial hydraulics. This elegant conciſeneſs deſerves ſo much the more praiſe, as his countrymen in general appear too often to make a merit of prolixity; and we ſhall have occaſion to remark, that beſides the convenience of ſimplicity, he has ſometimes been fortunate enough to unite with it the advantage of ſuperior accuracy.

Part 1. Mechanics.

The firſt part of the work is but ſhort; it relates to proper mechanics, and has little that is remarkably new or intereſting. In treating of pendulums, the author informs us, with reference to another work of his own, that the Rhinland or Brandenburg foot contains 139.13 French lines. Hence it appears that 100 Rhinland feet are exactly 103 Engliſh; and in this paper, the meaſures will be reduced accordingly.

Part 2. Hydraulics.

The ſecond part, relative to hydraulics, contains beſides a ſhort introduction, twenty-four chapters, almoſt every one of which preſents to us ſomething of importance.

Water iſſuing from reſervoirs.

Chapter 1. Of the motion of water flowing out of reſervoirs, and of the contraction of the ſtream.

Law of its velocity: As the ſquare root of the height.

§ 89. The velocity of water flowing out of a horizontal aperture, is as the ſquare root of the height of the head of water.

That is, the preſſure, and conſequently the height, is as the ſquare of the velocity: for the quantity flowing out in any ſhort time, is as the velocity; and the force required to produce a velocity in a certain quantity of matter in a given time, is alſo as that velocity; therefore the force muſt be as the ſquare of the velocity. The propoſition is fully confirmed by Boſſut's experiments; the proportional velocities, with a preſſure of 1, 4, and 9 feet, being 2722, 5436, and 8135, inſtead of 2722, 5444, and 8166; a very inconſiderable difference.

Another developement.

There is another mode of conſidering this propoſition, not mentioned by Eytelwein, which is a very good approximation. Suppoſing a very ſmall cylindrical plate of water immediately over the orifice, to be put in motion at each inſtant by means of the preſſure of the whole cylinder ſtanding on it, and ſuppoſing all the gravitation of the column to be employed in generating the velocity of the ſmall cylindrical plate, neglecting

 its

its own motion, this plate would be urged by a force as much greater than its own weight as the column is higher than itself; and this, through a space shorter in the same proportion than the height of the column. But where the forces are inversely as the spaces described, the final velocities are equal. (Young's Syllabus, 35.) Therefore the velocity of the water flowing out must be equal to that of a heavy body falling from the height of the head of water, which is found very nearly by multiplying the square root of that height in feet by 8, for the number of feet described in a second. Thus a head of 1 foot gives 8; a head of 9 feet, 24.

The well-known circumstance of the contraction of the stream or vein of water running out of a simple orifice in a thin plate, reduces the area of its section at the distance of about half its diameter from the orifice, from 1 to .660 or .666 according to Bossut, to .631 according to Venturi, and to .64 or $\frac{16}{25}$ according to the author's own experiments: hence the diameter is reduced to $\frac{4}{5}$. The contraction of the stream.

The quantity of water discharged is very nearly, but not quite, sufficient to fill this section with the velocity due, or corresponding to the height: for finding more accurately the quantity discharged, the orifice must be supposed to be diminished to .619, or nearly $\frac{5}{8}$. Hence we may multiply the square root of the height by 5 instead of 8, for this mean velocity in a simple orifice. Practical rule for a simple orifice. Multiply the square root of the height by 5 gives the velocity in 1 second.

If we apply the shortest pipe that will cause the stream to adhere every where to its sides, which will require its length to be twice its diameter, the discharge will be about $\frac{13}{16}$ of the full quantity, and the velocity may be found by taking $6\frac{1}{2}$ for a multiplier. Additional pipe in length twice the diameter of the hole requires a multiplier of $6\frac{1}{2}$.

The greatest diminution is produced by inserting a pipe so as to project within the reservoir, probably because of the greater interference of the motions of the particles approaching its orifice in all directions: in this case the discharge is reduced nearly to a half. Disadvantage of an interior pipe;

A conical tube, approaching to the figure of the contraction of the stream, procured a discharge af .92; and when its edges were rounded off, of .98, calculating on its least section. conical, &c. produced a discharge of 0.98.

Venturi * has asserted that the discharge of a cylindrical pipe Venturi's conical tube;

* Venturi's treatise is given entire in our Journal, 4to, Vol. II. and III.

may

may be increafed by the addition of a conical tube nearly in the ratio of 5 to 2; but Mr. Eytelwein finds this affertion fomewhat too ftrong, and obferves, that when the pipe is already very long, fcarcely any effect is produced by the addition of fuch a tube. He proceeds to defcribe a number of experiments made with different pipes, where the ftandard of comparifon is the time of filling a given veffel out of a large refervoir, which was not kept always full, as it was difficult to avoid agitation in replenifhing it, and this circumftance was perfectly indifferent to the refults of the experiments. They confirm the affertion that a compound conical pipe may increafe the difcharge to twice and a half as much as through a fimple orifice, or to more than half as much more as would fill the whole fection with the velocity due to the height: but where a confiderable length of pipe intervenes, the additional orifice appears to have little or no effect.

Increafes the difcharge from a fimple aperture twice and a half.

The chapter concludes with a general table of the coefficients for finding the mean velocity of the water difcharged by the preffure of a given head under different circumftances.

Multipliers for determining the velocity under different circumftances.

For the whole velocity due to the height, the coefficient, by which its fquare root is to be multiplied, is 8.0458.

For an orifice of the form of the contracted ftream, 7.8.

For wide openings, of which the bottom is on a level with that of the refervoir; for fluices with walls in a line with the orifice; for bridges with pointed piers, 7.7.

For narrow openings, of which the bottom is on a level with that of the refervoir, for fmaller openings in a fluice with fide walls, for abrupt projections and fquare piers of bridges, 6.9.

For fhort pipes from two to four times as long as their diameter, 6.6.

For openings in fluices without fide walls, 5.1.

For orifices in a thin plate, 5.

Chapter 2. Of the difcharge of water by horizontal and by fmall lateral orifices, in a veffel continuing full.

Particular cafes.

The principles detailed in the firft chapter are here applied to particular cafes.

Difcharge through a notch in the dam,

Chapter 3. Of the difcharge by rectangular orifices in the fide of a refervoir, extending to the furface.

The velocity varying nearly as the fquare root of the height, may here be reprefented by the ordinates of a parabola, and the

quantity of water difcharged by the area of the parabola, or two thirds of that of the circumfcribing rectangle. So that the quantity of water difcharged may be found by taking two-thirds of the velocity due to the mean height, and allowing for the contraction according to the form of the opening, as explained in the firft chapter.

has nearly two-thirds of the velocity due to the mean height.

The author has found this mode of calculation fufficiently near to the refults of Buat's experiments, and to fome accurate obfervations of his own.

He propofes for example a lake in which a rectangular opening is made without any oblique lateral walls, 3 feet wide, and extending 2 feet below the furface of the water. Here the coefficient of the velocity, corrected for contraction, is 5.1, and the corrected mean velocity $\frac{2}{3}\sqrt{2}\times 5.1=4.8$; therefore the area being 6, the difcharge of water in a fecond is 28.8 cubic feet, or nearly four hogsheads.

Particular exemplification.

The fame coefficient ferves for determining the difcharge over a were of confiderable breadth; and hence it is eafy to deduce the depth or breadth requifite for the difcharge of a given quantity of water. For example, a lake has a were 3 feet in breadth, and the furface of the water ftands at the height of 5 feet above it: it is required how much the were muft be widened in order that the water may be a foot lower. Here the velocity is $\frac{2}{3}\sqrt{5}\times 5.1$, and the quantity of water $\frac{2}{3}\sqrt{5}\times 5.1\times 3\times 5$; but the velocity muft be reduced to $\frac{2}{3}\sqrt{4}\times 5.1$, and then the fection will be $\frac{\frac{2}{3}\sqrt{5}\times 5.1\times 3\times 5}{\frac{2}{3}\sqrt{4}\times 5.1}=$ $\frac{\sqrt{5}\times 3\times 5}{\sqrt{4}}=7.5.\sqrt{5}$; and the height being 4, the breadth muft be $\frac{7.5}{4}\sqrt{5}=4.19$ feet.

Chapter 4. Of the difcharge from refervoirs with lateral orifices of confiderable magnitude, with a conftant head of water.

By lateral orifices of confiderable magnitude,

This may be found by determining the difference in the difcharge by two open orifices of different heights: but in moft cafes the problem may be folved with nearly equal accuracy, by confidering the velocity due to the diftance of the centre of gravity of the orifice below the furface.

the velocity due to the center of gravity of the orifice may be taken.

Chapter 5. Of the difcharge from refervoirs receiving no fupply of water.

For

Refervoirs not fupplied.

For prifmatic veffels, all the particulars of the difcharge may be calculated from the general law that twice as much would be difcharged from the fame orifice if the veffel were kept full during the time which is required for its emptying itfelf. (Young's Syllabus, 245). Where the form is lefs fimple, the calculations become intricate, and are of little importance.

Divided refervoirs.

Chapter 6. Of the difcharge from compound or divided refervoirs.

The author obferves from Buat, that the difcharge through an orifice between two refervoirs, below the furface, is the fame as if the water ran into the open air. Hence he calculates the difcharge when the water has to pafs through feveral orifices in the fides of as many refervoirs open above. In fuch cafes, where the orifices are fmall, the velocity in each may be confidered as generated by the difference of the heights in the two contiguous refervoirs, and the fquare root of the difference will therefore reprefent the velocity; which muft be in the feveral orifices, inverfely as their refpective areas; fo that we may calculate from hence the heights in the different refervoirs when the orifices are given. Mr. Eytelwein then confiders the cafe of a lock which is filled from a canal of an invariable height, and determines the time required, by comparing it with that of a veffel emptying itfelf by the preffure of the water that it contains, obferving that the motion is retarded in both cafes in a fimilar manner; and he finds the calculation agree fufficiently well with experiments made on a large fcale. The motion of water through different compartments of a clofed cavity is alfo determined.

A lock filled from a canal.

Velocity of a river confidered.

Chapter 7. Of the motion of water in rivers.

The fimple theorem by which the velocity of a river is determined, appears to be the moft valuable of M. Eytelwein's improvements; although the reafoning from which it is deduced is fomewhat exceptionable. The friction is nearly as the fquare of the velocity; not becaufe a number of particles proportional to the velocity are torn afunder in a time proportionally fhort, for according to the analogy of folid bodies, no more force is deftroyed by friction when the motion is rapid than when flow, but becaufe when a body is moving in lines of a given curvature, the deflecting forces are as the fquares of the velocities (Young's Syllabus, 40, 35, 46), and the particles of water in contact

contact with the ſides and bottom muſt be deflected, in conſequence of the minute irregularities of the ſurfaces on which they ſlide, nearly into the ſame curvilinear path, whatever their velocity may be.

At any rate we may ſafely ſet out with the hypotheſis, that the principal part of the friction is as the ſquare of the velocity. And the friction is nearly the ſame at all depths: for Profeſſor Robiſon found that the time of the oſcillation of the fluid in a bent tube was not increaſed by increaſing the preſſure againſt the ſides, being nearly the ſame when the principal part of the tube was ſituated horizontally as when vertically. The friction will however vary according to the ſurface of the fluid which is in contact with the ſolid, in proportion to the whole quantity of fluid: that is, the friction for any given quantity of water will be as the ſurface of the bottom and ſides of a river directly, and as the whole quantity of water in the river inverſely: or ſuppoſing the whole quantity of water to be ſpread on a horizontal ſurface, equal to the bottom and ſides, the friction is inverſely as the height at which the river would then ſtand, which is called the hydraulic mean depth. Elementary poſition, and inductions.

Now when a river flows with an uniform motion, and is neither accelerated nor retarded by the action of gravitation, it is obvious that the whole weight of the water muſt be employed in overcoming this friction; and if the inclination vary, the relative weight or the force that urges the particles along the inclined plane, will vary as the height of the plane when the length is given, or as the fall in a given diſtance (Young's Syllabus, 54); conſequently the friction, which is equal to the relative weight, muſt vary as the fall, and the velocity which is as the ſquare root of the friction, muſt be as the ſquare root of the fall; and ſuppoſing the hydraulic mean depth to be increaſed or diminiſhed, the inclination remaining the ſame, the friction would be diminiſhed or increaſed in the ſame ratio; and therefore in order to preſerve its equality with the relative weight, it muſt be proportionally increaſed or diminiſhed by increaſing the ſquare of the velocity in the ratio of the hydraulic mean depth, or the velocity in the ratio of its ſquare root. We may therefore expect that the velocities will be conjointly as the ſquare root of the hydraulic mean depth and of the fall in a given diſtance, or as a mean proportional between theſe two lines. Taking two Engliſh miles for a given length, we muſt The velocities will be as a mean proportional between the hydraulic mean depth and the fall;

muſt find a mean proportional between the hydraulic mean depth and the fall in two miles, and inquire what relation this bears to the velocity in a particular caſe, and thence we may expect to determine it in any other. According to Mr. Eytelwein's formula, this mean proportional is $\frac{11}{10}$ of the velocity in a ſecond.

or 11-10ths of the velocity per ſecond.

Confirmation by a practical inſtance.

In order to examine the accuracy of this rule, we may take an example which could not have been known to Mr. Eytelwein. Mr. Watt obſerved, as Profeſſor Robiſon informs us in the article River of the Encyclopædia Britannica, that in a canal 18 feet wide above, and 7 below, and 4 feet deep, having a fall of 4 inches in a mile, the velocity was 17 inches in a ſecond at the ſurface, 14 in the middle, and 10 at the bottom : ſo that the mean velocity may be called 14 inches, or ſomewhat leſs, in a ſecond. Now to find the hydraulic mean depth, we muſt divide the area of the ſection, 2. $(18+7)=50$ by the breadth of the bottom and length of the ſloping ſides added together, whence we have $\frac{50}{20.6,}$ or 29.13 inches: and the fall in two miles being 8 inches, we have $\sqrt{}(8\times 29.13)=15.26$ for the mean proportional, of which $\frac{10}{11}$ is 13.9; agreeing exactly with Mr. Watt's obſervation. Profeſſor Robiſon has deduced from Buat's elaborate theorems 12.568 inches for the velocity, which is conſiderably leſs accurate.

Another inſtance.

For another example we may take the Po, which falls 1 foot in two miles, where its mean depth is 29 feet; and its velocity is obſerved to be about 55 inches in a ſecond. Our rule gives 58, which is perhaps as near as the degree of accuracy of the data will allow.

On the whole, we have ample reaſon to be ſatisfied with the unexpected coincidence of ſo ſimple a theorem with obſervation: and in order to find the velocity of a river from its fall, or the fall from its velocity, we have only to recollect that the velocity in a ſecond is $\frac{10}{11}$ of a mean proportional between the hydraulic mean depth and the fall in two Engliſh miles. This is however only true of a ſtraight river flowing through an equable channel.

True only in a ſtrait river, &c.

Slope of the banks.

For the ſlope of the banks of a river or canal, Mr. Eytelwein recommends that the breadth at the bottom ſhould be $\frac{2}{3}$ of the depth, and at the ſurface $\frac{10}{3}$: the banks will then be in general capable of retaining their form. The area of ſuch a ſection

is

is twice the square of the depth, and the hydraulic mean depth $\frac{2}{3}$ of the actual depth. He then investigates the discharge of a canal of which the bottom is horizontal. The velocity appears in this case to be somewhat greater than in a similar canal, of which the bottom is parallel to the surface.

Velocities when the channel is curved

The author remarks that the velocity is greater near the concave than the convex side of a flexure: a circumstance probably occasioned by the centrifugal force accumulating the water on that side. No general rule can be given for the decrease of the velocity in going downwards: but sometimes the maximum appears to be a little below the surface. In the Arno the velocities are at 2 feet below the surface, $39\frac{1}{2}$ inches; at 4, $38\frac{1}{2}$; at 8, 37; at 16, $33\frac{1}{2}$; at 17, 31. In the Rhine at 1 foot, 58 inches; at 5, 56; at 10, 52; at 15, 43. As an approximation to the mean velocity, the author directs us to deduct from the superficial velocity $\frac{1}{130}$ for every foot of the whole depth. For instance; if the depth were 13 feet, and the superficial velocity 5 feet, to take $4\frac{1}{2}$ as the average velocity of the whole river. This can however only be true in large rivers: for in the canal, measured by Mr. Watt, the superficial velocity must be diminished nearly $\frac{1}{5}$ for a depth of only 4 feet. And we may in general come quite as near to the mean velocity by taking $\frac{9}{10}$ of the superficial velocity; although this may still differ materially from the true medium. But comparing this with the former theorem for the velocity, which gives a result oftener above than below the truth, we may bring them both into a form easily recollected, thus;

at different depths.

Concise deduction of the hydraulic mean depth.

The superficial velocity of a river is nearly a mean proportional between the hydraulic mean depth and the fall in two miles; and the mean velocity of the whole water is, still more nearly, nine-tenths of this mean proportional.

We may find a double confirmation of these principles in Major Rennel's account of the Ganges (Phil. Transf. 1781, p. 87).

Instance in the Ganges.

He informs us that "at 500 miles from the sea, the channel is 30 feet deep when the river is at its lowest; and it continues at least this depth to the sea," that "a section of the ground, parallel to one of its branches, in length 60 miles, was taken by order of Mr. Hastings;" that "the windings of the river were so great as to reduce the declivity on which the water ran, to less than 4 inches per mile;" that "the medium

rate of motion of the Ganges is lefs than three miles an hour in the dry months;" that is, its fuperficial velocity. Now allowing a little for the banks or fhelving fides, we may take exactly 30 feet as the hydraulic mean depth; then if the fall in two miles were precifely $\frac{2}{3}$, we fhould have $\frac{2}{3} \times 30 = 20$; and $\sqrt{20} = 4.47$ for the velocity in a fecond, or 3.05 miles in the hour: which is a little greater than the obferved velocity, becaufe the fall was affumed fomewhat too great.

Again (p. 110), "the river when full, has thrice the volume of water in it, and its motion is alfo accelerated in the proportion of 5 to 3. We may affume, that the hydraulic mean depth is doubled at the time of the inundation, whence the velocity will be increafed in the ratio of 7 to 5: but the inclination of the furface is probably fomewhat increafed at the fame time, which may eafily be fuppofed to increafe the velocity ftill further, from 1.4 to 1.7.

The effects of weres, falls, and contractions in rivers.

Chapter 8. Of the difcharge and the fwell in the cafe of weres, falls, and contractions, in rivers and canals.

The methods employed in the third chapter require here fome modification, fince the water arrives at the place of defcent with a confiderable velocity; and it is evident from mechanical, as well as from hydraulic confiderations, that the ultimate velocity will exceed that which is due to the depth of the ftream at the place of its defcent, and that it will correfpond to a height equal to the fum of the heights capable of producing thefe velocities. Hence we may calculate the effect of a bar in elevating the furface of a river; how broad a were muft be, in order to produce a certain elevation, and how much water will run over a given were according to collateral circumftances. When a bar is below the level of the lower water, we muft confider the difference of the two levels as conftituting the fall; the whole of the ftream below the level of the lower water deriving its additional velocity from this difference only.

The extent of the fwell produced by a given elevation of the furface of a river in confequence of the effect of a were or bar, may be determined by calculating from the rules for finding the velocity of rivers, the inclination neceffary for producing a given difcharge; the depth being greater, the inclination immediately above the bar will be lefs, but the effect of the fwell does not terminate at the point where the new furface,

4 if

if ſtraight, would have met the original ſurface; for on account of the rounding off of this angle, it extends nearly twice as far. The effect of reducing the breadth of a river may be determined in a manner nearly ſimilar. The author remarks, that a conſiderable diminution of breadth produces but a ſmall elevation, a reſult which appears to be conformable to experience; but that where depth is required for navigation, it may often be obtained by a projection built out from the bank, which may be ſufficient to increaſe the river's velocity, and to cauſe it to excavate its bed.

(To be concluded.)

VI.

Experimental Proof, that Corrections deduced from the Arcs of Vibration of a Pendulum in Vacuo, are practically uſeful. By Mr. Ezekiel Walker.

To Mr. NICHOLSON.

SIR, *Lynn, Aug.* 20, 1802.

YOUR annotation on my laſt paper * convinces me, that I ought to have given you my reaſons, derived either from theory or experience, for troubling you on the ſubject which it contains.

Introduction.

Though theory may be too uncertain a guide to be depended on, in a matter ſo complicated as the vibration of a pendulum in a variable reſiſting medium, yet I think that ſome philoſophical arguments might be advanced in its favour; but theſe I ſhall not at preſent enter into, as I mean to confine myſelf to mere matters of experience.

Arguments might be offered in favour of applying the theory of pendulums to clocks;

The clock which I uſed formerly was made by the late Mr. John Arnold, in his beſt manner. It had a dead beat, with a compound pendulum that vibrated in the arc of a circle. This pendulum frequently varied in its arc; and as the ſhort vibrations were performed in leſs time than the long ones, I was led to ſuppoſe that it might be governed by the ſame law, which

but at preſent the author confines himſelf to experiment or fact.

* See Vol. III. p. 273.

 obtains

Clock of Arnold's make with dead beat and compound pendulum, found to vary according to the theory.

obtains in a pendulum vibrating in a non-refifting medium by the force of gravity: and, on making ufe of corrections deduced from that principle, I found them agree fo well with the going of the clock, as left me no reafon to doubt of the truth of my conjecture. The table which I fent you was therefore conftantly ufed to correct the clock's rate, when any alteration was obferved in the arc of vibration; but the rate itfelf was afcertained by tranfits of the fun and fixed ftars over the meridian.

It is thought that the corrections would not be applicable in fome other combinations.

How far this table may be applicable to other clocks I know not, as this is the only inftance in which I have had any experience. It is evident, however, that this theory cannot be ufed to advantage where a pendulum is acted upon either by cycloidal checks, a faddle-piece, or pallats of a particular conftruction.

I am, SIR,

With much refpect,

Your humble fervant,

E. WALKER.

VII.

Defcription of an Apparatus of Tubes for facilitating to driving of Copper Bolts into Ships. By Mr. RICHARD PHILLIPS*.

Mr. Phillips's method of driving copper bolts, &c.

MR. RICHARD PHILLIPS, of Briftol, in feveral letters fent to the Society, ftates, that he had invented a method of driving copper bolts into fhips, without fplitting the heads or bending them; and that by means of tubes contrived by him for the purpofe, this could be effected without difficulty, and had been fatisfactorily executed in the prefence of feveral of the principal fhip-builders of Briftol.

A certificate accompanied thefe letters, from Mr. William James, and Mr. Samuel Haft, fhip-builders, and alfo from Mr. George Winter, of Briftol, teftifying that they had tried the

* From the Tranfactions of the Society for the Encouragement of Arts, &c. for 1801. The Society adjudged a reward of forty guineas to the inventor. Models are in their repofitory.

the experiment of driving copper bolts through the jointed cylinder invented by Mr. Phillips; and that they fo far approve of it, that they mean to adopt the general ufe of them, for driving bolts in all directions, particularly on the outfide of fhips, whether iron or copper; as this method not only prevents the bolts from bending, but keeps the heads from fplitting, and enables the bolts to be driven much tighter, than by any other means with which they were acquainted. They further add, that by the application of Mr. Phillips's cylinder and punch, a copper bolt which had been crippled at the edge of the hole, and which could not be ftarted by a mall, went up with eafe in a perpendicular direction in the flat of a fhip's bottom, not four feet from the ground.

Evidence of its utility.

This certificate was witneffed by Mr. William Holden.

The fame facts are alfo certified by Mr. Thomas Walker, and Mr. James M. Hillhoufe, of Briftol, who add their opinion, that the adoption of this invention in the different dock-yards of the kingdom, will prove very advantageous.

Since Mr. Phillips's firft application to the Society for a premium, he has made a confiderable improvement in the conftruction of his tubes. The defcription and engraving hereunto annexed are of the improved kind: models of both are, however, preferved in the Society's repofitory, for public infpection.

Defcription.

The inftrument employed for driving the bolts, confifts of a hollow tube formed from feparate pieces of caft iron, which are placed upon the heads of each other, and firmly held thereto by iron circles or rings over the joints of the tubes. The loweft ring is pointed, to keep the tube fteady upon the wood. The bolt being entered into the end of the hole bored in the wood of the fhip, and completely covered by the iron tube, is driven forward within the cylinder by an iron or fteel punch, placed againft the head of the bolt, which punch is ftruck by a mall: and as the bolt goes further into the wood, parts of the tubes are unfcrewed and taken off, till the bolt is driven home into its place up to the head.

The tubes are about five inches in circumference, and will admit a bolt of feven-eighths of an inch in diameter.

Reference

Reference to the Engraving of Mr. R. Phillips's Method of driving Bolts into Ships. Plate III. Fig. 3, 4, 5, 6.

Fig. 3.

A. The copper bolt, with one end entered into the wood, previous to fixing the tube.

B. A piece of timber, or ſhip's ſide, into which the bolt is intended to be driven.

Fig. 4.

CCCC. The parts of the iron tube faſtened together, ready to be put on the bolt A.

DDDDD. Iron or braſs rings with thumb-ſcrews, placed over the joints of the tube, to hold them firm together.

EEEEE. The thumb-ſcrews, which keep the rings and tubes firm in their proper places.

F. Two points formed on the lower ring: they are to ſtick into the timber, and to enable the tube to be held firm in its place.

Fig. 5.

Shows the ſeparation of the parts of the tube, which is effected by ſlackening the thumb-ſcrews and rings.

To put them together, you ſlide the rings over the joints, placed as cloſe as poſſible; then, by tightening the thumb-ſcrews, you will have them firm together, and may continue the tubes to any length, from one foot to whatever number is required.

Fig. 6.

GH. Two ſteel punches or drifts, to be placed on the head of the copper bolt within the tube, whilſt driving. The blow given upon the punch drives forward the bolt. The ſhorteſt of them ſhould be uſed firſt, and, when driven nearly to its head, ſhould be taken out of the tube, and the longer punch applied in its place.

VIII. *Reply*

VIII.

Reply to Dr. YOUNG'S *Letter on the Theory of Compound Sounds. In a Letter from Mr.* JOHN GOUGH.

To Mr. NICHOLSON.

SIR,

YOU have publiſhed Dr. Young's anſwer to my remarks on the theory of compound ſounds; and I doubt not but you will, with equal impartiality, find a place for the following reply in your Journal. When a controverſy is conducted with good humour, it affords amuſement; and when it furniſhes freſh facts, or places the ſubject of diſpute in a new point of view, it proves inſtructive. Dr. Young's anſwer poſſeſſes both theſe qualities; and it is my duty to imitate him, at leaſt in the obſervance of the former. Having now completed what may be called the introduction of the preſent letter, I will, with the permiſſion of Mr. Nicholſon, make a few remarks upon the principal objections contained in the anſwer under conſideration.

On the proper ſpirit of controverſy.

The Doctor obſerves, that my theory of vibrations in an elaſtic fluid differs as widely from Dr. Smith's hypotheſis as from his own conceptions relating to the ſame operations: but he will probably recollect, that I profeſs to maintain compound ſounds to be mixtures of elementary ſounds, not aggregates by coaleſcence; in other words, I undertake to defend Dr. Smith's propoſition, as a fundamental maxim both of harmonics and the general philoſophy of ſounds. When the queſtion is thus properly ſtated, the engagement I undertake to perform cannot in any ſenſe oblige me to ſupport the collateral arguments, with which Dr. Smith thought proper to elucidate his propoſition, perhaps unfortunately. For if the poſitive maxim, *viz.* that compound ſounds are mixtures, be eſtabliſhed, the deſign of my eſſay is accompliſhed: and as Dr. Young does not controvert the leading concluſions of this paper, I may fairly ſuppoſe he admits them, but am at a loſs to underſtand in what manner they favour his ſide of the queſtion; for if my inferences be juſt, compound ſounds are not formed by coaleſcence, which is the point in debate. I do not pretend to ſay preciſely what would be the conſequence of conducting a number of ſounds through

Explanatory remarks.

That compound ſounds are mixtures,

and not formed by coaleſcence.

through a narrow tube, becaufe I do not recollect a circumftance of the kind; nor do I think myfelf bound to determine the effect, becaufe an inftance, which is fo particular, can have no weight in a difpute concerning a general principle. I am acquainted, however, with one circumftance which deferves attention at prefent: the notes of two or more bells are not obliged to coalefce by paffing through the narrow found-holes of the fteeple of a country church: and if Dr. Young will fuppofe the point A, in the demonftration of my third propofition, to be placed in the mouth of his tube, he will fee that the motions of the corpufcle will not be difturbed by the edge of the pipe. The Doctor's anfwer blames my reprefentation of his idea of a compound found, as amounting to a charge of ignorance in the moft common occurrences. The reprefentation appeared to be a neceffary introduction to the objections which were levelled, in the fequel of my effay, at the Doctor's theory, not at his experience; and if the ftatement be not juft, it is difficult to difcover the fenfe of the term coalefcence in that fection of his paper in which the fubject is examined, efpecially when the author infers, that the ftrength of the found in a concert is not in exact proportion to the number of inftruments compofing it. The writer of this paragraph, without doubt, wifhed his readers to conclude, that the vibrations, which are communicated to the air by a number of founding bodies, reduce themfelves to a fingle fet of vibrations by mutual oppofition: Now my objections to this doctrine muft remain in force, until mankind can be convinced that unity poffeffes the qualities of number.

Fact refpecting bells, of which the founds are faid not to coalefce.

The anfwer corrects the new theory in two effential points: *Firft,* It gives a power of analyzing compounds to the ear; but this power cannot be admitted before the doctrine of coalefcence is eftablifhed: *Second,* It reduces the coalefced compound to a pigmy, and joins to it the powerful reflected parts of its conftituents, thereby forming a mixture. A few remarks on the range of fuch a mixture fhall clofe this letter. When a traveller approaches a town, he frequently hears the bells in it at the diftance of four or five miles; the beating of a drum may catch his attention at one-third of that diftance; when near the place, he perceives the blows of hammers, and at laft a mixed noife. In his journey, then, he meets with a fucceffion of founds at the limits of their refpective ranges, but does not find the

On the mixture of founds differing in their ranges.

the coalefced compound in the van of the train. If the preceding ftatement be natural, the range of a mixture of founds muft be eftimated by means of the ftrongeft individual in the aggregate, not from the coalefced compound, admitting its exiftence. This conclufion holds good in a general fenfe; at the fame time I know, that the ranges of certain aggregates exceed the ranges of their ftrongeft conftituents. Of this kind is a general difcharge of fire-arms; but though I admit the fact, it does not appear to be an inftance of coalefcence. For the report of every mufket employed is nearly alike; and the explofion of the whole number, at the fame moment, agitates a large field of air. Now I know, by obfervation, that when an extenfive furface is made to vibrate it founds to a great diftance; for when the fhear-men of Kendal beat the tenters on a calm morning, the ftrokes may be heard for two miles, or more; though the ear of a by-ftander is not much affected by them, the operation being performed by ftriking the ftretched cloth with a ftick not thicker than a man's finger. Seeing then a number of founds which are nearly in unifon, forms an aggregate of more power than any one of its conftituents, it follows, that when a multiplicity of inftruments contains a ftrong combination of unifons, that combination determines the range of the concert.

Sounds of aggregates heard farther than their loudeft conftituents, mufquetry.

Shearmen beating the tenters heard two miles.

JOHN GOUGH.

Middlefhaw, near Kendal, Auguft 19, 1802.

IX.

A new Procefs for Claying Sugars, propofed by CIT. HASSEL LACHENAIE, *Chief Apothecary of the Military Hofpitals of Guadaloupe to the Agents of the Confuls of the French Republic in the Windward Iflands.*

(Concluded from Vol. II. Page 190.*)*

THE third and laft which I fhall mention, though the leaft of the inconveniencies of the procefs of Citizens Boucherie, cannot be reconciled with the economy which we are obliged to ufe in the employment of our time; namely, the neceffity of graining the fugar before it is put into the cafes, &c.

Economy of time.

After

Procefs of the author.

After having pointed out the principal difficulties and inconveniences which would refult in the manipulation of the claying of fugar, by the ufe of cafes conftructed after the model of thofe of Citizens Boucherie, nothing more remains to be done, than to fhew how I have fucceeded in overcoming thefe difficulties, by ufing cafes fabricated according to my own principles.

Conftruction of the cafes or receptacles for claying fugars, defcribed by reference to the drawings.

My cafes are without a bottom; they are conftructed of four boards, united in a fquare by tenons and mortices, which are held clofely together by wedges and pins, fo as to form a fquare veffel, each fide of which is three feet long, infide meafure. This cafe therefore prefents a furface of nine fquare feet. The fide *h* (fee Plate XII. Vol. II.) is 18 inches high at its extremity *e*, and 16½ inches at its other extremity *f*. The fide *l*, parallel to *h*, is cut in the fame proportions. The height of the fide *k* correfponds with that of the extremity *l*; and the fide *i*, has the fame height as the extremity *e*.

I place four cafes, of the conftruction juft defcribed, upon a frame eight feet long, or plank, *m*, *m*, *m*, *m*, having a fall of three inches, and raifed two feet above the ground *q q*. This board ferves as their bottom. In the fpace occupied by each cafe, it is perforated with twenty holes of an inch in diameter, placed in four rows: thefe holes, which I ftop beneath with pins projecting eight inches above the bottom within, ferve for the drainage, when, after the fugar has cooled, thefe fame pins, which perforate it almoft to the half of its thicknefs, are drawn out.

For receiving the fyrup, I place below the board feveral channels which convey it into a common trough intended to conduct it into a refervoir. Thirty-two cafes difpofed in this manner upon eight boards, in my fugar work, will fupply the place of 900 forms, and as many pots.

It is evident that by thus inclining the board which ferves as a bottom to the cafes, it was indifpenfibly requifite that I fhould give the fame inclination to the lower edges of the fides, in order that this upper part might be at the fame level; and this inclination was the more neceffary, as it favours the running off of the fyrup at the borders, where the cafes are in contact with the board, no lefs than is done by the holes with which it is perforated.

After

After defcribing the conftruction and difpofition of the cafes which I propofe, I fhall now point out the manner in which I ufe them.

In order to avoid very fatiguing tranfportations, in which much time would be loft, I have eftablifhed a cooler in my fugar-work, into which the boiled fugar is conveyed by a gutter from the battery. Near this cooler I have placed two backs, each of the fame capacity with one of my cafes, and thefe laft are all of equal dimenfions. Manufacturing procefs.

MANIPULATION.

I draw off my fugar by two batteries *; when thefe are joined together in the cooler †, I pour them into one of the backs where the fugar cryftallizes by cooling. This fugar is ftirred two or three times during the interval, until two other batteries have been poured into the refrigeratory: I then fill up my back, and with a paddle I mix well together the fugar thus collected; after which I leave it at reft till it acquires the confiftence it is fuffered to affume, when intended to be manufactured into raw fugar. In the interval other fugar is made, to be poured in the fame manner into the fecond back. The fugar firft cooled in a back,

As foon as the fugar has acquired the before-mentioned confiftence, I ufe fmall buckets for conveying it into one of my cafes, which I fill to the height of fix lines below its margin. I then leave it till coated, at which period I draw out the pegs from the board, in order that the fyrup may run out. then put into the cafes to drain,

On the next and the following days, one of the fides of the cafes may be opened to obferve how the drainage goes on; but it muft immediately be replaced. This facility, which has the advantage of afcertaining the moft favourable moment for claying the fugar, enables us alfo to afcertain at pleafure the effect easily examined to afcertain the time for claying.

* The battery (*batterie*) is the veffel in which the boiling of the fugar is terminated, and when two boilings are united in the refrigeratory, it is cuftomary in our manufactories to call this procefs *drawing off the fugar by two batteries (tirer le fucre par deux batteries, l'enformer par deux batteries)*. Thefe two batteries conftitute the filling (*emplie*). The name of *battery*, which ferves to defignate this boiler, is given to it becaufe the fugar is beaten with a paddle, or ftirrer, whenever it tends to raife itfelf above the fides.

† This is a large boiler which receives the boiled fugar, and in which it is fuffered to lofe its heat before it is drawn off.

effect of the claying. When, by examining the fugar in this manner, we find that there does not remain more than about two inches difcoloured by the fyrup, it is the proper time for claying it.

Preparation for the claying.

To prepare the fugar for receiving the clay, the very thin cruft formed at its furface is firft taken off, and is divided and fpread out again upon this furface, which is pierced to the depth of fix lines with the edge of the trowel, which is afterwards ufed for flattening and compreffing the bottom. After this laft operation is performed, it is clayed in the ufual manner.

Qualities of the clay.

The fat earth or clay which is generally ufed here for claying of fugars, has in general the fault of retaining the water with which it is diluted too long, on which account it happens, that whilft one part of this water filtrates into the fugar, another, and that not the leaft confiderable, evaporates into the air.

Defect from tenuity.

This earth has alfo another fault, which is very prejudicial to the operation of the bleaching of the fugar; namely, that of remaining for feveral days in part fufpended in the water, before it is intirely precipitated from it, which renders this fluid milky. Both thefe faults require to be corrected.

After fome trials I fucceeded in doing this completely: my procefs is fimple, eafy to be executed, and requires neither expence nor lofs of time; I can even affert, that the water with which my earth thus prepared is diluted, depofits nothing by filtrating into the fugar, but that it feparates as limpid as the pureft rain water. I fend you fome fugar bleached by this means.

Advantages refulting from the Ufe of my Cafes and Manipulation for the claying of Sugar.

FIRST ADVANTAGE.

With Regard to the Weight.

Statement of advantages in this new apparatus and procefs.

By cafing the fugar in the manner here defcribed previous to fubjecting it to the claying procefs, I prevent the formation of the *fountain (la fontaine)* *, and of the thick cruft with which every form prefents us. Now, not having to feparate the

* This is defcribed at page .

fountains

fountains of the twenty-fix forms which each of my cafes contains, the quantity of the fugar of thefe fame fountains remains therefore in the mafs. This mafs alfo fuffers a much fmaller lofs in the claying operation, than it would have done if this operation had been performed upon twenty-fix feparate forms.

Lefs cruft on the furface.

SECOND ADVANTAGE.

With Refpect to the Regularity and Uniformity of the Grain.

When fugar is made to cryftallize in our forms, the cryftals that are fucceffively precipitated from it, diminifh alfo fucceffively in volume in rifing towards the top of the form which, in its pofition, then reprefents an inverted cone. In order to avoid this inconvenience, it is cuftomary to ftir the fugar as foon as its cruft becomes folid. Neverthelefs, a fountain is almoft always formed, and for this reafon: when the mafs contained in our large forms begins to cool, the confiftence which its ftill liquid part affumes, in proportion as it lofes the caloric by which it was liquefied, refifting the force of attraction which tends to unite the cryftalline particles fymmetrically together, in order to conftitute regular cryftals, thefe particles are precipitated, and apply themfelves confufedly together, fo as to produce the folid cruft which conftitutes this fountain.

The grain very uniform.

The greater uniformity of cryftallization in the mafs which my cafes contain, (though I have difturbed it by agitating it feveral times), compared with what happens in the forms which have not been moved, and even in thofe which have been, with equal care and dexterity, is obtained at the moment when I cafe my fugar. The confiftence which it has acquired no longer permits its cryftals to obey the laws of ftatics; but the fmaller being confounded with the larger, the whole prefents a perfect uniformity, not to be found in thofe forms which have been moved. For in them this operation is performed at a moment when there ftill exifts a fufficient degree of liquidity to permit the larger cryftals to obey the fame laws.

Caufe why the grain is better.

The reafon why no fyrup is collected above the fugar depofited in my cafes, depends on the feparation of the cryftals, which being not at all agglutinated together, leave intervals fufficiently large to receive it. But as this fyrup ftill retains much of its heat when the fugar is cafed, it depofits a fufficient number of cryftalline particles in cooling to agglutinate the cryftals, and to caufe the whole mafs to fettle by its own weight

No fyrup collected at the top.

weight in proportion as the ſyrup runs off, and acquire a force of adheſion eq‿al to that of the ſugar put into forms.

THIRD ADVANTAGE.

As to its Whiteneſs or Colour.

and much leſs at the baſe.

The caſed ſugar bleaches as well under the clay, as that which is put into forms, and its baſe retains proportionally leſs ſyrup than the head of the form.

FOURTH ADVANTAGE.

With Reſpect to the Manipulation.

COMPARISON. 1. *Leſs Labour and loſs of Time.*

Numerical ſtatement of the ſaving of labour.

When we fabricate ſugar, we employ every day a man to waſh the forms, to carry them to the ſugar houſe, and to plant them.

By the uſe of the caſes, I avoid the neceſſity for employing this man, and the loſs of this time.

For putting the ſugar into forms, two men carry it and divide it in the forms, which they fill. When theſe forms are ſufficiently cool, they carry them into the refining houſe, in order to put them upon pots into which their ſyrups drain.

For caſing the ſugar, it is tranſported, one ſingle time, by ſmall buckets into my caſes, which are cloſe to the backs. This operation is much leſs fatiguing than that of the forms, which weigh about 80 lbs. at the moment when they are thus conveyed.

After ſome days drainage, the bottom of each form is deeply perforated, in order to extract the fountain which is ſeparated from it. When this buſineſs is finiſhed, the forms are again carried to other pots, deſtined to receive their fine ſyrup, after which the bottom is diſpoſed for the claying.

When, on opening one of the ſides of my caſes, the ſugar is found to be in the proper ſtate for claying, its bottom is prepared as I have deſcribed, page , after which the diluted earth is poured upon it.

In treating 416 forms in this manner, four men generally work 51,711 hours, which, multiplied by 4, amounts to 206,844 hours work for one man.

In this manner four men clay 16 caſes, which contain as much as 416 forms, in the ſpace of 2,840 hours, which multiplied by 4, amounts to 11,360 hours work for one man.

Thus

Thus the labour of claying 416 feparate forms, with refpect to the time employed in doing it, is to that for the fame number of forms contained in 16 cafes, as 206,844, to 11,360, or as 51,711 to 2,840. Now therefore, in order to clay the quantity of 416 forms of fugar in my 16 cafes, (reckoning the day's work of each man at nine hours) I employ only one day and 2,360 hours, whereas the claying of 416 feparate forms requires 22 days and 8,844 hours.

The new method of claying performed in about one twentieth part of the ufual time.

I find alfo another faving of time and labour when the earth is to be removed and the bottom cleaned. This work, which requires a day's work of four men for the 416 forms, is performed in half a day by one man, upon the fame quantity of fugar in the cafes. I have made this comparifon with my watch in my hand.

2. *Fewer Difficulties in making the Bottom.*

Though our cultivators are in the habit of working the forms and making their bottom, it frequently happens that they neglect to remove the fountain completely; frequently alfo they do not uniformly comprefs the grained fugar which they had taken out of the form, in order to feparate this fountain, and which they replace in it before making the bottom. In that cafe, the filtration of the water of the clay does not take place uniformly, but meets with obftacles towards fome of its fides; and the bottom, after this claying, exhibits diftinct marks of the defective manipulation.

In my cafes, as there are no fountains to be feparated, as the cryftallization is uniform throughout the whole mafs, the furface of which is level before the cooling of the fugar, it is fufficient that this furface be very flightly opened before levelling the bottom: this bufinefs is very eafily performed, and thofe workmen to whom I have confided the execution for the firft time, have perfectly fucceeded. The uniformity of the cryftallization in my cafes, alfo favours that of the filtration of the claying water into the mafs of the fugar.

The bottom is levelled more eafily.

3. *Another faving of Time and Labour.*

The forms being fufficiently drained, the fugar is taken out in order to be conveyed to the

When I find the fugar fufficiently drained in my cafes, I take them down, and leave them

The drying and ftoving is performed with more facility and effect.

ſtove, where the loaves are depoſited upon frames, after the head, which is ſtill wet with ſyrup, has been ſeparated from them. For, without this precaution, it would flow back into the maſs, diminiſh its whiteneſs, and render the ſugar more liable to attract the moiſture of the atmoſphere. Some days afterwards theſe forms are cut in pieces, in order to complete the deſiccation of the ſugar, and to ſeparate the whiteſt parts from the more coloured.

them for ſome days expoſed to the air, in order to deprive them of part of their humidity which is retained by that of the forms, when carried immediately to the ſtove: I afterwards break them into large pieces *, and at the ſame time I ſeparate the whiter ſugar from the reſt. By this means the workmen are ſaved the labour of dividing the ſame ſugar, as is commonly done, in a hot ſtove which frequently muſt not be ſuffered to cool without the danger of injuring the quality of the ſugar, and which, on account of the length of time they remain in it, diſpoſes them to contract dangerous diſeaſes.

FIFTH ADVANTAGE.

The laſt of the advantages which I ſhall mention, and which is not the leaſt important, is that of *economy*.

It is very expenſive, and ſometimes difficult to procure the forms and pots; but this apparatus is cheap and eaſily had.

It is known that it would coſt an immenſe ſum to thoſe who, like myſelf, being without forms and pots, are under the neceſſity of providing themſelves with them, ſince it is almoſt impoſſible to procure them, now that the Saints and Martinique are in the power of the enemy.

With little expence, I conſtruct and eſtabliſh 32 caſes, which ſupply the place of 802 forms and as many pots, and by this means avoid the great loſſes occaſioned by the daily rupture of theſe veſſels.

I think it unneceſſary to recapitulate the obſervations contained in this memoir, and only wiſh that you may conſider them as having ſome title to your approbation.

* Which are likewiſe carried into the ſtove, but which dry more ſpeedily than the loaves.

X. *Deſcription*

X.

Description of a Water Wheel. By Mr. J. BESANT. *In a Letter to Mr.* CHARLES TAYLOR, *Secretary to the Society for the Encouragement of Arts. From their Transactions.*

SIR,

I BEG leave to lay before the Society some observations respecting the common Undershot Water-wheel, and to point out the superiority of that of my invention.

The common undershot wheel

First,—In common water-wheels more than half the water passes from the gate through the wheel, without giving it any assistance.

lets much water pass without effect.

Secondly,—The floats coming out of the tail-water are resisted with almost the whole weight of the atmosphere, at the instant they leave the surface of the water.

The floats are resisted at their rise,

Thirdly,—The same quantity of water which passed between the floats at the head, must of course pass between them at the tail, and consequently impede the motion of the wheel.

and they tend to lift the tail water.

In the water-wheel of my invention,

First,—No water can pass but what acts, with all its force, on the extremity of the wheel.

In the new wheel

Secondly,—The floats coming out of the water in an oblique direction, prevent the weight of the atmosphere from taking any effect.

the floats are oblique.

Thirdly,—Although the new water-wheel is heavier than that on the old construction, yet it runs lighter on its axis, the water having a tendency to float it.

It is in part buoyant;

Fourthly,—By experiments made with the models, proofs have been shown, that the new wheel has many advantages over the common wheel, and that, when it works in deep tail water, it will carry weights in proportion of three to one, so that it will be particularly serviceable for tide-mills.

and is good for tide mills.

I hope on trial, before the Society, my invention will prove successful, and am,

Sir,

Your obedient Servant,

No. 26, *Brompton.* J. BESANT.

To the SECRETARY of the
Society of Arts, &c.

Tried with fuccefs by the Society.

Repeated experiments of the above invention were made by the Committee; from the refult of which it appeared to poffefs fome advantages over the common wheel, and to have a greater power of action.

Defcription of the late Mr. Befant's Water-wheel. Plate III. Fig. 1 & 2.

Defcription of the engraving.

A. The body of the water-wheel, which is hollow in the form of a drum, and is fo conftructed as to be proof againft the admiffion of water within it.

B. The axis on which it turns.

C. The float boards, placed on the periphery of the wheel. Each board is obliquely fixed firm to the rim of it, and to the body of the drum.

D. The refervoir, containing the water.

E. The penftock, which regulates the quantity of water running to the wheel.

F. The current of water which has paffed the wheel.

Fig. 2. Is a front view of the water-wheel, fhewing the oblique direction in which the float boards C are placed on the face of the wheel.

XI.

Defcription of an Hydrometer which gives the Specific Gravity by Infpection, conftructed by Mr. ATKINS.—W. N.

Improvements in Atkins's hydrometer. The weights of different figures.

SINCE the publication of the defcription of Atkins's hydrometer in the laft Number, that artift has communicated to me two improvements in the fame. The firft confifts in making the four weights of different figures, namely, round, fquare, triangular, and pentagonal; and he ftamps the figure of the weight on the fliding rule, oppofite to every letter in the feries to which it belongs. By this contrivance, which indicates a confiderable degree of fagacity with regard to the practical requifites of an inftrument offered for general ufe, he renders it impoffible for the revenue officer to miftake one weight for another, or to take out his refult at a wrong part of the fliding rule.

The

The other alteration, though it will be highly acceptable to philofophical men, and renders the inftrument of more extenfive utility, and independent of the fliding rule, feems not fo clearly to be an amelioration with regard to its moft frequent and daily ufe. Inftead of the letters of the alphabet, the four faces of the ftem carry graduations which at once point out the fpecific gravity of the fluid according to the form now univerfally adopted; namely, by taking water as unity. Its figure is fhown in Plate IV. Fig. 1. and it is provided with three weights; the inftrument itfelf being ufed without a weight at the upper range of denfities.

Hydrometer having the fp. gravity graduated on its ftem.

Upon this occafion I cannot avoid noticing the ftrange overfight of philofophers and others, who have expreffed the denfities of acids and other fluids by certain graduations or degrees of inftruments, fuch as that of Baumé and others; the comparative values of which were either little known to the public, or ill determined; fo that a very effential part of what we read in books of chemiftry is to many readers void of meaning. This is the more remarkable, fince the expreffion of fpecific gravities before mentioned has been long eftablifhed, and is quite as eafy to be put upon an inftrument as any other notation. It is now feven years fince I urged this matter in my chemical dictionary at the article Hydrometer, and the evil ftill remains. If my recollection be accurate, I think Briffon made the fame propofal many years ago in the Memoirs of the French Academy. In the prefent inftance, Mr. Atkins gains the advantage of fimplifying his fliding rule by leaving out the alphabet, fo that the operator looks for the fpecific gravity in the firft inftance, and upon the oppofite lines he finds the ftrength and concentration.

Remarkable overfight of philofophical chemifts in noting fpecific gravities by the degrees of fome particular inftrument.

Long ago noticed by the author, and by Briffon.

Advantages in fimplifying Atkins's fliding rule;

But ftill more it is worthy of notice, that if he poffeffed the inftrument only, or fhould in preference be defirous of referring to Gilpin's tables, or any other elementary courfe of experiments, he can proceed immediately from the inftrument to the table without ufing the fliding rule to determine his fpecific gravity, as he muft with the other.

and rendering the inftrument applicable to all tables, &c.

I was at firft very ftrongly impreffed with the facility of this inftrument in giving a refult which ordinarily demands to be computed, and I fuppofe the advantages above ftated will fecure it the preference with many. But upon reflection, I cannot but think the alphabetic inftrument intitled to the preference

But the alphabetic hydrometer appears better adapted for dif-

patch and security against unskilfulness.

ference for commercial purpofes, efpecially in the hands of men unufed to make experiments, or to read the graduations of a rule. It is fo much more eafy to pitch upon a fingle letter than to read a graduation by ftroke, as well upon the inftrument as the fcale, that I apprehend the literal inftrument, with the varioufly figured weights, will in general be attended with greater certainty in the hands of all but philofophers. And for this reafon the inventor is undoubtedly right in offering both to the public.

Cylindric bulb.

The cylindrical figure of the bulb affords the means of diminifhing the fize of the affay jar, and confequently will demand a lefs quantity of the fpirit to make the requifite trials.

XI.

On the Air from Finery Cinder and Charcoal, with other Remarks on the Experiments and Obfervations of Mr. Cruickfhank. In a Letter from JOSEPH PRIESTLEY, *L. L. D. F. R. S. &c. &c.*

To Mr. NICHOLSON.

DEAR SIR, *Northumberland (America), Ap.* 16, 1802.

MR. CRUICKSHANK having added a fupplement to the account of his experiments on the air from finery cinder and charcoal, in anfwer to my objections to the new theory from the properties of that kind of air, I have given more particular attention to it, and wifh you to add the following obfervations to thofe which I have already fent you on the fubject.

The air from finery cinder and from charcoal ftated to differ from other denfe inflammable air only in the proportion of fixed air it affords by combuftion.

When I firft procured this kind of air, I was far from imagining that I had difcovered any *new* fpecies of air, effentially different from any other, fo as to be entitled to a new appellation, but only another variety in the heavy inflammable air, which is known to be exceedingly various in different proceffes, and even in the different ftages of the fame procefs, all however agreeing in this, that when all fixed air is carefully wafhed out of them, more is found on the decompofition of them when they are fired together with dephlogifticated air. And

And in this eſſential property the air from finery cinder and charcoal agrees with them all, differing only in the proportion of the fixed air procured in this manner. This, however, I now find muſt be called *the gazeous oxide of carbon*, while the others are called *hydro-carbonates;* that being ſaid to conſiſt of two parts oxigen to one of carbon, and the others to be a ſolution of carbon in hidrogen, or the light inflammable air.

It cannot, however, be denied, that this gazeous oxide of carbon is *inflammable* as well as the hydro-carbonates, in the compoſition of which a portion of hidrogen (one of the component parts of water) is a neceſſary ingredient. This air, therefore, from finery cinder and charcoal, though called an *oxide*, muſt be eſſentially different from all the other oxides, none of which are combuſtible, being ſubſtances already ſaturated with oxigen. Thus iron is a combuſtible ſubſtance, ready to unite with oxigen when preſented to it in a proper temperature; but when it is ſaturated with oxigen, and therefore called *an oxide of iron*, it is no longer combuſtible. It muſt, therefore, as it appears to me, be an abſolute abandonment of one of the moſt fundamental principles of the new theory, to call the air from finery cinder and charcoal an oxide. If ſubſtances be combuſtible in proportion to their affinity to oxigen, and their conſequent readineſs to unite with it, this air, which is inflammable, muſt be of *this* claſs, and therefore the very reverſe of the oxides, which are ſaturated with oxigen, and incapable of receiving more.

It is inflammable as well as the hydro-carbonates, and cannot with any conſiſtency be called an oxide, becauſe it is combuſtible.

If this kind of air was a real oxide, it would appear to be ſo on the decompoſition of it; when, to make the reſult unexceptionable, the oxigen it contained would either take the form of dephlogiſticated air, or become a component part of ſome other ſubſtance into which oxigen was acknowledged to enter. But this has not been done. When it is decompoſed by being fired together with dephlogiſticated air, the fixed air which is then formed comes, I have no doubt, from the oxigen in the dephlogiſticated air, and the phlogiſton in this ſpecies of inflammable air; the ſame being the reſult, though not quite in the ſame degree, of firing the heavy inflammable air from charcoal and water, from oil, &c. &c. into which it is not pretended that any oxigen enters.

Its oxigen has never been ſeparated or ſhewn by tranſpoſition, as happens with real oxides.

Mr.

Mr. Cruickſhank's obſervation of the union of the oxigen in the dephlogiſticated marine acid air with inflammable air in the common temperature of the atmoſphere, is extremely curious but I do not ſee that any thing more can be inferred from it, than from the more rapid union of the ſame principles when inflammable and dephlogiſticated air are fired together.

Objection to Mr. Cruickſhank's theory of the reduction of finery cinder and charcoal.

Mr. Cruickſhank ſays, p. 201, that "the oxigen in the finery "cinder, uniting with the carbon in the charcoal, forms fixed "air; and that the metal being in the ſame proceſs revived, "decompoſes this fixed air, when it becomes again to a cer-"tain degree oxigenated." But why ſhould not the finery cinder retain a part of its oxigen, rather than firſt part with it, and then take it again? Beſides, it is not true that after this proceſs the iron is in any degree oxigenated, for it is completely revived, being perfect iron; and that any fixed air is either formed or decompoſed in this proceſs is altogether conjectural, and for the reaſons that I have given cannot be admitted. For though it might be poſſible for oxigen in the finery cinder (ſuppoſing it to contain that principle) to be extracted from it by its affinity with the carbon in the charcoal, and that nothing ſhould enter in its place, the iron thus revived could not decompoſe the fixed air that would be formed by their union.

Not doubting but that Mr. Cruickſhank will give a candid attention to theſe obſervations,

I am,

DEAR SIR,

Your's ſincerely,

J. PRIESTLEY.

XII. *Deſcription*

XIII.

Description of Mr. READ'S *Pneumatic Apparatus. By a Correspondent.*

To Mr. NICHOLSON.

SIR,

I BEG leave to recommend to your notice a very simple and cheap apparatus for displacing any elastic fluid out of a reservoir by means of water. It is described in Dr. Beddoes's Considerations on the Medicinal Use of Factitious Airs, octavo, London, 1796; and I hope you will think with me, that it deserves a corner in your respectable publication. Notice of a very simple apparatus for the gases.

I is a funnel with its pipe or tube stopped at B, and perforated above and below with two or three small holes. Round the tube A is soldered the tube D, left open at top. The tube G is made to circumscribe the tube D, and to be soldered to the funnel at F, and likewise soldered at E. Description.

When water is poured into the funnel, and the cork K K inserted into a bottle filled with gas, the water descends through the tube A till it arrives at the division or section B; it then flows through the small holes at C, and ascends between the tubes A and B; flows over the top of D, and descends again between the tubes D and G, till it arrives at E (where it regains the tube through an opening above E), and thence into the bottle. As the water goes in, the air escapes between the tubes A and G, through the cock H, into the mouth-piece. The lower end of G is made to perforate a cork which is properly attached to it, and which secures it air-tight into the neck of a common bottle, as seen at fig. 3. Effect.

The whole apparatus may be made for less than a guinea, and may be introduced to the bed-side of the patient, if necessary, by having a cask filled instead of a bottle, and placed upon a chair. By turning the cock the patient may take an inspiration whenever he pleases, the cock being thrust into the spiggot hole. It is very cheap.

I am, &c.

N. N.

SCIENTIFIC

SCIENTIFIC NEWS, &c.

Experiment of the Invifible Girl.

Defcription of the apparatus for the experiment of the Invifible Girl.

MONS. Charles, brother to the well known philofopher of that name, has for fome time exhibited in Leicefter-fquare an experiment called the Invifible Girl, which feems to include fome principle of acouftics either newly difcovered, or not hitherto known to the extent there difplayed. In the middle of a large lofty room in an old houfe, where from the appearance of the wainfcot, the pofition of fome glafs cafes of natural hiftory that occupy one fide, and from other circumftances, there does not appear to be any fituation for acouftic tubes or reflectors—is fixed a wooden railing about four feet high, and five feet wide, inclofing a fquare fpace. From the four corner pofts of this railing there rife painted wires, or rods, to an eminence of ten or twelve feet, (the cieling being much loftier) and from the upper ends of thefe hangs, by ftrings refembling the lines ufed with curtains, a fquare glazed box, having a mahogany bottom nearly on a level with the top of the railing, and in the middle of this box is fufpended an hemifphere (or larger portion) of tin or pewter, out of which proceed horizontally four tin trumpets at right angles to each other, and about eighteen inches long. The difk or hemifphere is covered with a portion of a quickfilvered glafs globe which completes the fphere, and clofes it imperfectly, fo as not to prevent the inner aperture of one of the trumpets from being feen through the place of infertion. This whole apparatus is fo far detached from all furrounding objects, that it can be fwung about as far as the railing will allow; that is to fay, the box can be fwung by its four ftrings, and the globe and trumpets by the ftring that fuftains them in the box. The globe may be about ten inches in diameter. In all thefe dimenfions, which I do not fuppofe to be material, I fpeak from the recollection of fome weeks ago.

Manner in which the voice iffues from the apparatus.

In the exhibition, the Invifible Girl is fuppofed or pretended to be included in the fphere, and the converfation is held by fpeaking into any one of the trumpets, and the reply comes out of them; that is to fay, it is moft clearly heard by applying the ear to one of their mouths. The voice is low, as if con-

veyed

veyed from a diſtance through a tube, but it is very diſtinct. The lady converſes in ſeveral languages, ſings, deſcribes all that happens in the room, and diſplays a fund of lively wit and accompliſhment, that admirably qualify her to ſupport the character ſhe has undertaken.

Experiment of ſound conveyed to great diſtances by a thread.

I hope to meet my reader hereafter with a little more to ſay on the nature of this experiment. At preſent I can only offer the conjecture that the ſound is conveyed through the ſtrings, and to this I am led by another experiment of the celebrated Dr. Moyes. He ſhewed me, that if one end of a packthread, or even ſewing thread, be wrapped round the finger, and that finger be put into the ear (not forcibly) while the other ear is ſtopped, and at the ſame time an aſſiſtant wraps the other end of the ſtring round the handle of a tuning fork, and (the ſtring being then drawn rather tight) he cauſes the fork to ſound, too low to be heard by himſelf and the byſtanders, it will nevertheleſs be very audible to him who holds the other end of the ſtring. We tried it at an interval of fifty feet, and the Doctor has tried it at upwards of three hundred, without any ſenſible diminution of the effect. I wrapped the ſtring round my wriſt, and put my finger in my ear, and found the effect very nearly as ſtrong. If inſtead of the tuning fork one obſerver bites hold of the ſtring between his fore teeth, while the other applies it as before to his ear by means of his finger, the voice of the former may be tranſmitted; but it is more difficult to manage this experiment with ſucceſs than the other.

Converſation through a ſtring.

Aeroſtatic Experiments of GARNERIN.

Revival of aeroſtation.

A new generation has ſprung up ſince the epoch at which Air Balloons conſtituted the object of aſtoniſhment and ſpeculative reſearch about twenty years ago. They who remember that buſy period as if it were yeſterday, muſt feel a little ſurprized at the viciſſitudes of human affairs, when they find how many of thoſe who are now intirely engaged in ſociety, were then ſcarcely in exiſtence; and how few have ſeen a balloon. The ſpectacle exhibited by Mr. Garnerin, though much leſs impreſſive than the experiments of his countrymen to whom this invention is due, in all their novelty and originality have nevertheleſs been received with much intereſt. The readers of our Journal will recollect him as the firſt adventurer whoſe intrepidity

Garnerin the firſt operator with a parachute.

intrepidity and ſkill * enabled him to deſcend through the atmoſphere by means of a Parachute; and thoſe who extend their reſearches to the new relations and conſequences of uncommon experiments, will probably conſider theſe daring attempts as the embryon efforts towards the art of flying; which ſo many eminent men have thought within the reach of human power. He has not, however, yet, in the uncertain climate of our iſland, been able to gratify us with the ſight of his deſcent in this manner. Three aerial voyages, perfectly reſembling thoſe of his predeceſſors, have conſtituted the whole of what he has yet been enabled to perform.

Three voyages in England.

The firſt with Capt. Sowden.

On the 28th of June he made his firſt aſcent from Ranelagh Gardens. After the promiſe of a fine day by the indications of the morning, the weather changed, and at five o'clock, when the whole was in readineſs, there was a ſtrong wind from the ſouth weſt, with occaſional ſhowers. At this hour Mr. Garnerin and Capt. Sowden aſcended, and proceeded over St. James's Park, the Thames, and the bridges of Weſtminſter and Blackfriars. After proceeding over the metropolis they threw out a quantity of ballaſt, and aſcended to an elevation which Mr. Garnerin eſtimates at upwards of 10,000 feet above the ſurface of the earth, or nearly two miles perpendicular. At the expiration of about half an hour the valve was opened, and the balloon deſcended through dark clouds which had intercepted their view of the earth, upon ſight of which they found themſelves ſpeedily advancing towards the ſea. On account of the tempeſtuous weather their landing was very dangerous and unpleaſant. The wind dragged them over fields and hedges, which tore their hands and cloaths, and when their anchor had engaged itſelf at laſt in a thicket near a houſe, the inhabitants were ſo far from aſſiſting them, that they even appeared diſpoſed to fire upon them. For want of this neceſſary help their cable broke, and they were dragged forward till the balloon was ſufficiently lacerated to begin to collapſe, and ſuffered them to jump out.

Aſcended two miles.

Dangerous landing.

Singular narrative of Captain Sowden.

Capt. Sowden publiſhed an account of this excurſion, in which he relates ſome ſingular obſervations, conſiderably differing from thoſe of former aeronauts. He affirms, that his ſight in looking down was ſo ſtrong, that he could eaſily diſtinguiſh

* Philoſ. Journal 4to. I. 523.

guiſh the minuteſt objects on the earth. In this vaſt Map, he could not only trace the different roads and interſections, but even the ruts and furrows in the field. His hearing became likewiſe ſurpriſingly acute. The rattling of the carriages on the roads, the lowing of cattle, and the acclamations of the populace, were diſtinctly heard at the height of fifteen thouſand feet; though at the ſame time the travellers could ſcarcely hear themſelves ſpeak. He is perſuaded that a perſon with a ſtrong voice and a ſpeaking trumpet, might be perfectly underſtood from the earth at that elevation in the air. Inſtead of finding the climate colder as he aſcended, he obſerved the contrary.

Account of their landing near Colcheſter, after travelling at the rate of 80 miles per hour.

The place of their deſcent was on a common four miles beyond Colcheſter and ſixty miles from Ranelagh, and the time from their departure was three quarters of an hour; conſequently they travelled at the rate of eighty miles an hour. They were very hoſpitably received by Mr. Kingſbery, a freeholder of the county, who imagined their viſit to be on account of the parliamentary election, in which he had determined not to interfere. By his very friendly ſervices they were refreſhed and proceeded to Colcheſter, whence they returned to London on the following day.

Second voyage, July 5, 1802.

The ſecond voyage was made on the fifth of July, from Lord's cricket ground, near Paddington. The very unfavourable weather rendered his aſcent much leſs ſtriking than it might elſe have been. Thouſands of individuals filled the ſurrounding fields for ſome hours before the aſcent, amidſt rain nearly inceſſant, and a continual haze. The balloon roſe with Mr. Garnerin, accompanied by Mr. Locker, at ten minutes before five in the evening, and in four minutes afterwards it was loſt in the maſs of clouds. At the end of the fifth minute they began to deſcend, and landed at Chingford in Eſſex, after having remained in the air for one quarter of an hour. The moſt remarkable obſervation in Mr. Locker's account is, that neither himſelf nor Mr. Garnerin could diſtinguiſh ſounds above the elevation of 3 or 4,000 feet.

Pyrotechnic balloon, July 20.

On the 20th following a balloon was ſet up from Vauxhall gardens in the night, having an extenſive ſyſtem of fire works attached to it, to be diſcharged by means of a fuze, at a great height in the air previous to the exploſion of the balloon itſelf.

4 This

This exhibition, which does not require any general philoſophical explanation, was attended with the moſt complete ſucceſs, and had a very ſtriking effect.

Third voyage, with Mrs. Garnerin and Mr. Glasfurd.

The laſt voyage was made from Vauxhall Gardens by Mr. Garnerin, accompanied by Mrs. Garnerin and Mr. Glasfurd, on Tueſday, the 3d of Auguſt, at a quarter paſt ſeven in the evening. The day was calm and ſerene, and Mr. Garnerin made the offer of deſcending by the parachute, which he ſays in his letter was rejected, but not by whom. Circumſtances were highly favourable to the proſpect. The travellers paſſed ſlowly over the metropolis in bright ſun-ſhine, at an elevation which permitted them to be diſtinctly ſeen by a good perſpective magnifying about 30 times, and for the moſt part they were much more clearly ſeen. A cat attached to a parachute was let fall from an height of about 600 feet. It deſcended ſwiftly at firſt, till the parachute was fairly expanded, and then came down ſlowly and with ſafety. They landed near Hampſtead, about two miles north of the ſkirts of the town.

Promiſe of a diſſertation on this object.

Notwithſtanding the very ample diſcuſſion the ſubject of aeroſtation has had at the period of its invention, I apprehend that my readers will not be diſpleaſed to ſee an outline of what relates to it, with an inquiry into the probabilities of its becoming of any extended degree of utility; and this I purpoſe to do in our next.

Commercial College at Hull.

Objects of a propoſed commercial college.

Dr. J. Alderſon of Hull, has given out propoſals for eſtabliſhing a college in that town, for purpoſes highly intereſting to that commercial intercourſe which has in ſo many reſpects extended the powers and enjoyments of the civilized ſocieties of modern times, and is ſo intimately connected with the improvement of the intellectual and moral habits of man. He ſtates as the objects of the eſtabliſhment, *1ſt*, To obtain for the man of buſineſs preciſe information on the nature and value of every article of commerce, whether crude or manufactured; *2d*, To point out the country where ſuch articles are beſt to be procured; *3d*, To render him acquainted with the various proceſſes by which they are rendered marketable; and, *4th*, To inſtruct him in the languages of the different countries to which the objects of commercial intercourſe may lead him.

The

The plan is to form a museum, and to erect a building for the accommodation of Professors, and the reception and display of specimens of raw materials and manufactured articles. One Professor to be engaged to teach the southern languages, another the northern, and a third to have the care of the museum and the office of explaining whatever relates to the specimens under his care. The estimate for the building and specimens, to be raised by subscription, is 2,500l.; and it is proposed that the corporations should endow the Professors with 50l. per annum each. Plan.

The Doctor, who stands in the honourable situation of proposer of an establishment so manifestly useful, disclaims the notion of this being a charitable institution; but considers it as the private interest of every subscriber. "As the father of a large family," he observes, "I certainly should not think myself at liberty to subscribe fifty pounds merely for the personal pleasure that results from the contemplation of a charitable action; but in subscribing fifty pounds to the above institution, I consider my interest and the welfare of my family. I am convinced it will add to the wealth and importance of the town; and in both of these I am deeply interested: but the greatest interest I have, is in the facility it will give to the education of my children. In a commercial country and a sea-port town, the acquisition of knowledge that evidently leads to the improvement of trade, becomes an object of the first moment in the education of youth; and the mode now proposed offers advantages which no private school can possess. To bring together under the immediate observation of youth (while yet the memory is most retentive), perfect specimens of the different articles of commerce, both of the raw material and of the manufactured, will enable him to bear in mind the precise value of any article he may be called upon to appreciate: To point out the country where every article is first procured or manufactured, will qualify him to go to the cheapest market, and by teaching him the language of such country, he will be at all times capable of transacting his own concerns without the intervention of interpreters, often a very serious source of imposition." Its great advantages.

It will no doubt be of great advantage to the town of Hull, that this plan should meet with support, and to the community at large, that it should be followed in other places.

2 *A System*

A Syſtem of Chemiſtry in Four Volumes. By THOMAS THOMSON, *M. D. Lecturer on Chemiſtry in Edinburgh.*

Thomſon's chemiſtry.

This work of Dr. Thomſon (who is well known to the public by his excellent notes on an edition of Fourcroy's chemiſtry in three volumes, as well as by ſeveral important memoirs in our Journal, and other productions) is intended to exhibit a detail of the vaſt number of facts which conſtitute the ſcience of chemiſtry, blended with the hiſtory of their gradual developement and of the theories which have been founded on them, and accompanied with exact references to the original works in which the different diſcoveries have been regiſtered. His plan or order of arrangement will be beſt ſeen by enumerating the contents.

After a ſhort introduction, it is divided into parts, books, chapters, and ſections. Part firſt relates to the principles of chemiſtry. Simple ſubſtances, viz. oxigen; ſimple combuſtibles, viz. ſulphur, phoſphorus, carbon, hidrogen, azote: Metals, 22.—Light; caloric; general obſervations.—Compound ſubſtances. Primary: Alkalis; earths, 10; oxides; acids. Secondary: Glaſs, ſalts, hidro-ſulphurets, ſoaps.—Of affinity.—Examination of nature. Of the atmoſphere; of water; of minerals; of vegetables; of animals.

To enter into a detail of the numerous intereſting and highly valuable facts and ſtatements which abound under the heads above enumerated, would be impracticable in the ſhort limits of a notice. I muſt therefore content myſelf with obſerving, that it is a clear, comprehenſive, and accurate ſyſtem, in which the learner will be orderly led from one truth to another, and the man of information will with eaſe find the compendium and references he may want; that it is beautifully and cloſely printed on fine paper, with ſide notes; and upon the whole muſt prove a deſirable acquiſition to our ſtock of means for the advancement of chemical knowledge.

Death of THOMAS GARNET, *M. D. late Profeſſor of Chemiſtry and Natural Philoſophy at the Royal Inſtitution, &c. &c.*

Death of Dr. Garnet.

On the 28th of June laſt died of a typhous fever, caught in the gratuitous exerciſe of his profeſſion, Dr. Thomas Garnet, a man who, for his philoſophical knowledge, his aſſiduous exertions in diffuſing information by his lectures, and his amiable

amiable and unaſſuming diſpoſition in ſociety, will be long regretted by the world, and by his friends in particular. He was a native of Weſtmoreland, and after a regular education, ſtudied phyſic at Edinburgh, which he afterwards practiſed at Harrowgate. Subſequent to this period, he commenced a courſe of lectures on chemiſtry and experimental philoſophy at Liverpool, which he repeated at Mancheſter with ſuch ſucceſs, that when a vacancy offered in Anderſon's Inſtitution at Glaſgow, he became a candidate for the appointment, and obtained it. In this ſituation he acquired ſo high reputation, that he was applied to by the Managers of the Royal Inſtitution to be the firſt Lecturer in that eſtabliſhment, which unſolicited honour he accepted, and delivered a very extenſive and truly laborious courſe of lectures during the commencing ſeſſion of that corporate body, which were followed by the firſt ranks in audiences ſo crowded, as perhaps were never before witneſſed on a ſimilar occaſion.

Some account of his life.

Before he quitted Harrowgate he married a Miſs Cleveland, whoſe talents and accompliſhments were peculiarly adapted to his own. With this amiable friend and partner of his fortunes he paſſed a few ſhort years happily, and became the father of two daughters. But the birth of the latter infant was fatal to her mother. It was ſoon after this event, and during the early exertion of his fortitude under ſo great an affliction, that he became attached to the Royal Inſtitution. His conſtant attention to his numerous occupations, ſupported by the hope of ſecuring an independence for himſelf and the orphan repreſentatives of their regretted parent, afforded the moſt rational means of conſolation. But the energies of the mind cannot ſupport the phyſical ſtructure under accumulated labours, anxious reflections, and the ſolitary privation of that domeſtic ſociety which choice and habit had rendered dear to the human heart. His health became impaired, but his determination to proceed, continued in all its vigor. He took an houſe in Marlborough ſtreet, increaſed his ſtock of apparatus, completed a lecture room, and his claſſes became highly reſpectable under him as the lecturer on his own private account. At this period it was, when he had applied the whole of his means and the utmoſt ſtretch of his powers to the well grounded plan of eſtabliſhing himſelf in the uſeful and honourable profeſſion he

Some accounts of Dr. Garnet.

he had adopted; at this period it was that death terminated all his hopes, and left his infants too young to feel the poſſible extent of their misfortune!

I have ſaid the *poſſible,* and not the *probable* extent of their misfortune. He himſelf in the lateſt moments of his life muſt have known—nay he did know—that the public he had endeavoured to ſerve and to inſtruct, have ever been alive to caſes much leſs powerfully claimant on their feelings, their generoſity, and their juſtice. Theſe children cannot be forſaken by the numerous and affluent individuals who eſteemed his virtues and revered his talents. When it ſhall be known that his Lectures on Zoonomia are to be publiſhed by a ſubſcription deſtined to the purpoſe of affording them an humble, but independent ſupport, they will moſt aſſuredly find the loſs of their parent ſupplied in this reſpect, by that ready benevolence which is ſo peculiarly characteriſtic of our country. It is intended to addreſs the Public at the ſeaſon when the town fills; but in the mean time the Royal Inſtitution have permitted the work to be printed at their preſs, and have ordered a donation worthy of the ſpirit and liberality of their views. Several gentlemen have alſo ſubſcribed, and I am authorized to aſſure the friends of this laudable undertaking, that ſubſcriptions are received at the houſe of the Royal Inſtitution. I ſhall myſelf be very happy to afford any farther information to enquirers.

***** Mr. Notlem will have the goodneſs to point out the Volume and Page of the paſſage for which he wiſhes to have an explanation.**

A
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AND
THE ARTS.

OCTOBER, 1802.

ARTICLE I.

Observations upon some remarkable Wells near the Sea Coast at Brighthelmstone, and other Places contiguous. By the Rev. W. Pearson, *P. R. I. From the Author.*

IN Fisher's "Description of Brighthelmstone, and the adjacent country," (p. 38, fourth edition) it is said that "*water* is procured at Brighton from wells of considerable depth, and being filtered through chalk, of which these parts are one immense rock, it is of course perfectly cleared of every species of foulness. The following phenomenon is observable in several of these wells; *at the time of high water they are empty, and at low water they are full;*" likewise under the article *Rottingdean* of the same pamphlet we read, "On the road to Newhaven, at the distance of near four miles from Brighthelmstone, lies the pleasant and delightful village of Rottingdean. This place is remarkable for its wells, which *are nearly empty at high water, but which rise as the tide declines.* These assertions appeared on perusal so singular, that I formed a determination of availing myself of the opportunity which a temporary visit to this fashionable bathing place gave me in the month of July last, of examining how far the statements in question are *accurate;* accordingly I obtained a plumb line, and

The wells near Brighthelmstone are said to be empty at high water, and full at low water,

at Rottingdean.

and meaſured the depths of the *ſurface* and *bottom* reſpectively of ſeveral wells in different weeks, and at different hours of the tide; the reſult of which meaſurements turned out to be a convincing proof that this traditionary report, though partially founded in fact, has been greatly exaggerated; but whether or not for the purpoſe of entertaining the viſitors of this place, it would be preſumptuous to determine. At the time, however of my viſit, it was not my intention to lay before the philoſophical reader any obſervations which were made, but merely to regiſter them by way of private amuſement; which intention would have been adhered to, had I not met with a paragraph in the General Evening Poſt of Tueſday, Aug. 24, 1802, of which the annexed is an exact copy.—"Rottingdean, four miles from Brighton, on the road to Newhaven, is remarkable for the ſingular variation of its wells, which *riſe as the tide declines, and are nearly empty at high water!* The reſearches of philoſophy have not as yet been able to affix the cauſe of this apparent contradiction in the courſe of nature." This attempt to excite a more general aſtoniſhment than was excited by the Brighthelmſtone guide alone, and conſequently to call the attention of the philoſopher to inveſtigate the ſubject before us, has induced me to recur to my notes, and to tranſmit for your Journal the deductions I had made from them, in order to ſhow that there is no *apparent contradiction* in the *phenomenon,* as it has been called, of the Brighton Wells.

This report is not accurate, but was lately repeated in a reſpectable newſpaper.

The South Downs, which conſtitute a ridge of high ground to the north of Brighton, the ſummit of which is parallel to the ſea coaſt, have their *gradual* ſlope inclining towards the ſea or ſouth ſide, and the declivity, which approaches more to a perpendicular line, facing the level country on the north; and the *dip* of the ſtratum of chalk, and conſequently of its impermeable bed, is in a direction from the ſummit to the ſea, or from north to ſouth; the rain therefore which falls upon theſe Downs may be expected to run down the bed of the rocks until it arrives at their lower extremity, where, if it meet with any oppoſing clay, or other impermeable ſubſtance, it will collect into a body of water, and, as it collects, riſe through the crevices of the rock and ſuperincumbent permeable earth, till it ſhews itſelf in the form of ſprings. This reaſoning, if my recollection be perfect, is agreeable to the theory of Mr. Pilkington, whoſe ſucceſs in draining and tapping ſprings

The South Downs lie on a bed of chalk ſloping towards Brighton; upon which the rains are conveyed; and form ſprings at its lower extremity;

ſprings is now pretty generally known; and in conformity to it I was led to ſearch in the firſt place for the lower extremity of the rock, which I found was *upon the ſhore,* near the ſeparation between the rounded ſtones and ſand, at from thirty to forty yards by eſtimation below the bank. Here, as I had previouſly ſuppoſed, I found the freſh water continue to ſpring or guſh out conſtantly in a line extending parallel to the bank, both to the eaſt and weſt of Brighton as far as I examined. In this line then are ſituated the *natural ſprings* of the place, which, if the ſea were remote, would be not only perennial, but probably would emit an equal * quantity of water at all times; but in every tide the ſea covers this line of natural ſprings, from three to ſix feet we will ſuppoſe, according to the ſtate of the moon, wind, &c. ſo that the freſh water is prevented from making its eſcape by the heavier ſuperincumbent ſalt water of the ſea, the conſequence of which is, that, from the time the ſalt water covers this line, the freſh water begins to accumulate in the bed of the rock and inclined rock itſelf, as high at leaſt as the ſurface of the tide. During the time therefore that the tide continues above this line, the freſh water is pent up in the rock at a diſtance ſufficiently remote from the ſea, to exhibit a perceptible riſe in the artificial wells ſouthward of the bank, which are dug down into the bottom of the rock. That this ſhould be the caſe to a certain extent it was natural to expect, but *what period of time ſhould elapſe* between the inſtant of the tide's arriving at the line of the natural ſprings, and the inſtant of a perceptible riſe commencing in the artificial wells, at ſome diſtance from that line, can only be aſcertained by actual obſervation, for both the diſtance of the wells, and number as well as capacity of the ſmall natural ducts that convey the water muſt be taken into the conſideration, if an attempt were made to inſtitute a calculation, and I confeſs my memoranda are rather deficient in this reſpect; my attention having been directed more particularly to aſcertain the ſtates of the tide when the wells were at the two extremes of riſing and falling, which times are erroneouſly aſſerted to be about ſix hours from being contemporary with the

which make their way into the ſea at the diſtance of thirty or forty yards below the high-water bank.

Theſe ſprings are covered by the ſea every tide;

and conſequently the upper water is prevented from coming down;

and the wells become fuller;

not at *high water* but *later.*

Obſervations as to the facts.

* The reaſon for this ſuppoſition is, that the quantity of inclined ſurface is very conſiderable, and is alſo contiguous to the ſea, where ſhowers are frequent.---W. P.

influx and efflux of the tide. The two wells, which were moſt convenient for examination, were, one immediately behind Fiſher's library, and another in a little office at a ſmall public houſe called the Dolphin, near the foot of Eaſt-ſtreet, which the fiſhermen uſe for procuring water for tanning their nets; which two were nearly equally diſtant from the bank, and ſubject to the ſame alternations of riſing and falling.

Deductions.

The deductions which were made from a variety of meaſurements were as follow:

The wells are fulleſt during the ebb.

1. The wells are moſt filled with water when the tide is retiring from the line of the natural ſprings; evidently becauſe the accumulation of freſh water continues as long as the ſalt water is above the ſaid line.

The ſprings are covered four hours by the ſea;

2. The whole continuance of the tide above the line of the ſprings, or time of filling, appears on an average about four hours.

and uncovered ſix hours.

3. The whole time of emptying ſeems to be about ſix hours.

A ſhallow well becomes quite empty; a deeper not ſo.

4. The well at the Dolphin is about three hours quite empty in every tide, but the other, which is deeper, is never quite empty.

Particulars, well known.

5. At two hours after high water on the 16th of July, viz. one day and a half after full moon, the water in the well at the Dolphin was two yards deep.

6. The fiſhermen who uſed the well at the Dolphin, informed me that they *knew very well* that the water in the wells roſe to the greateſt height at *two hours* after high water, and were at the loweſt *two hours* after low water, which information accords with my obſervations.

The water freſh.

7. The water always taſted freſh and pleaſant.

A remote well ſcarcely varies.

8. A well about 170 yards more remote from the ſea than that of the Dolphin, in a direction towards the market, never varied more than three inches and a half in depth at the different times of obſervation.

Why the deeper well holds its water.

9. The well near Fiſher's is eight yards and 30 inches deep, which depth is probably below the line of the natural ſprings, and therefore it contains water at all times; but the other is not ſo deep, and therefore loſes all its water every tide.

The effect has thus been correctly ſtated and accounted for.

From theſe deductions it will, I preſume, appear evident not only that the accounts which have been circulated reſpecting the wells at Brighton, Rottingdean, and other villages contiguous to the Suſſex coaſt, are erroneous, but alſo that a

ſatisfactory

fatisfactory reafon may be given for their reciprocation, which, though not contemporary with the ebbing and flowing of the tide, is yet fo far dependent upon it, as to follow it at an interval of two hours, but in fuch a way that the *immediate influence* of the tide upon it only lafts about four hours out of the twelve, notwithftanding the effect produced in that time requires nearly fix more to put the wells into their original ftate of vacuity or reft.

It refembles a river tide, produced on the frefh water by the fea: like that it is later; and has its ebb longer than the flood. If the ftream flowed down a declivity higher than the higheft tide, the ftream would always ebb.

Laftly, the fame reafoning which accounts for the alternate rifing and falling of the water in the artificial wells, makes it more than probable, that if the line of the natural fprings had been below the low-water mark, the artificial wells would have been not only required to be deeper, but would have been invariable in quantity; alfo, that if the faid line had been above high-water mark, it would have been in vain to have dug for water into the rock above, whilft there continued a natural iffue below: and fhould it be afked, why the water that has an immediate communication occafionally with the fea yet continues *frefh*, the fame anfwer may be given that will apply in the cafe of frefh water being driven back to Chelfea and Putney at every fucceffive tide that enters the mouth of the Thames.

Parfon's Green, Aug. 29, 1802.

II.

On the Theory of Galvanifm. By JOHN BOSTOCK, *M. D. From the Author.*

Galvanifm and electricity are the fame.

THE following Paper contains fome remarks upon the electrical pile of Volta, principally made with a view to illuftrate and confirm the theory of its action, which I propofed in my former communication.

1. With refpect to the nature of the agent producing the phenomena of galvanifm, there can fcarcely remain any reafonable doubt of its perfect identity with the electric fluid. The apparent diffimilarity in the effects of an electrical machine and a galvanic pile has been fufficiently accounted for, without fuppofing any thing effentially different in their nature, and we feem enabled by proper management to produce from either

But electricity is *produced* in the pile.

either apparatus at pleasure every effect of galvanism strictly so called, or of electricity. There appears however to be a great difference in their mode of action; in the pile the fluid is actually generated, whereas in the machine it is merely transferred from one body, where it previously existed in a disengaged state, to another body. In support of this position it may be alledged, that the energy of the pile is found to be materially increased by being insulated, whereas in the common machine it is impossible to produce any considerable degree of excitement *, unless there be a communication formed by a conductor between the rubber and the surface of the earth. Dr. Woollaston, who has thrown so much light upon this subject, seems to have carried too far the analogy between the methods of exciting the electric fluid, when he considers the oxidation of the amalgam on the cushion, as more than a secondary cause in the operation of the electrical machine. By considering the pile as a *generating*, and the other as only a *collecting* apparatus, we may assign a probable cause for the vivid sparks, the bright flashes, and the more considerable marks of excitement which are exhibited by the common machine; in this latter case a quantity of electric fluid derived from the neighbouring bodies, is as it were forcibly detained in the apparatus, and is ready to escape to every substance for which it possesses any degree of affinity; whereas in the pile, though there is a rapid and constant production of electricity, yet being derived from a change which takes place in its own composition, by which different parts of it appear to acquire different capacities for the electric fluid, it seems little disposed to enter into any combination with the surrounding bodies, and is only brought into action by the approximation of the different parts of the apparatus itself.

The pile is considered as a *generating*, and the electric machine as a *collecting* apparatus.

The effects of the pile has been ascribed to its great quantity of electricity at a low intensity;

It was justly remarked by Mr. Nicholson, that the peculiar effects of the pile may be ascribed to the electric fluid existing in it in a large quantity, but at a low degree of intensity. This consideration easily explains all the singular properties of the apparatus, but it still remains to be ascertained by what means

* This is an oversight. In fact, the ground serves only to form a communication between the receiving conductor and the cushion, and the excitation is fully as strong when the current is made to fly from the plus to the minus conductor, when both are insulated.---N.

means this difference of intenſity is produced. By different degrees of electrical intenſity it is generally underſtood, that the ſame quantity of uncombined * electric matter may be diffuſed through a greater or leſs ſpace, and in conſequence of this, as it exiſts in a ſtate of concentration or dilution, it will exhibit effects more or leſs violent. In the galvanic pile it might have been ſuppoſed from the ſmall bulk of the apparatus, that a large quantity of electric matter could not have exiſted in it except in a concentrated ſtate, but we find that the contrary is invariably the caſe; nor has any method been hitherto contrived by which the electricity of the pile can be made to exhibit the ſame marks of intenſity with that of the common machine. Future experiments will probably enable us to determine with more preciſion in what the difference between a greater or leſs intenſity conſiſts; poſſibly the electric fluid, as is the caſe with many other bodies, may differ in its nature according to the method employed for extricating it from the ſubſtances with which it was previouſly united.

but the nature of the differences of intenſity have not been explained.

2. That the extrication of the electric fluid in the pile of Volta depends upon the rapidity with which the ſurface of the metal is oxidated, after the experiments which have been performed, and particularly thoſe of Mr. Davy, ſeems to require no farther proof. It is however not only neceſſary that the fluid ſhould be extricated, but that when diſengaged it be concentrated and carried forward in a uniform direction. This conſideration clearly explains the reaſon why in the conſtruction of a pile there muſt either be two metals employed, which are differently acted upon by the interpoſed fluid, or that the different ſurfaces of the ſame metal be ſubjected to the action of different flnids. Dr. Van Marum conceives it impoſſible that the extrication of the electric fluid can depend upon the oxidation of the metals, becauſe he found that the pile acted more powerfully when he uſed the muriate of ammoniac, than when a diluted acid was employed; but the objection of the learned experimenter is ſufficiently obviated by the conſideration ſuggeſted above. He farther alledges in ſupport of his opinion, that a pile with potaſh interpoſed between the plates acted very powerfully, though no oxidation was produced. I have repeated this experiment, and have on the contrary uniformly

The electricity of the pile depends on the rapidity of oxidation, &c.

Dr. Van Marum denies this.

particularly becauſe potaſh afforded galvaniſm without oxidation; but the contrary proved to be the fact.

* Or uncompenſated by its oppoſite.---N.

formly found the ſurface of the zinc covered with a white cruſt; in this caſe the water employed in diſſolving the potaſh, oxidates the zinc in conſequence of the attraction which ſubſiſts between the oxide of zinc and the fixed alkali. We conclude it to be a general fact, that whenever a metal is oxidated the electric fluid is diſengaged, and it is probable that the reverſe of this propoſition will be found to be true, viz. that whenever electricity is diſengaged from a metal it becomes oxidated. It will appear therefore, that the experiments which were made with ſo much accuracy by Sig. Volta, upon the different electricities produced in metals by mutual contact, though certainly inadequate to form a complete theory of the action of the pile, are yet not to be diſregarded in our attempts to increaſe the power of the apparatus. In order that its conſtruction be adapted for exhibiting the greateſt energy, it will be neceſſary both that the metals employed be ſuch as to produce a current of electricity when brought into contact, and alſo that the interpoſed fluid ſhould rapidly oxidate one of the metals without exerciſing any action upon the other. The combination of zinc and ſilver anſwers the firſt of theſe conditions, and accordingly affords the beſt materials for the formation of the pile. With reſpect to the interpoſed ſolution, the muriates, and particularly the muriate of ammoniac, have for the moſt part been found the moſt efficacious, probably in conſequence of the attraction which ſubſiſts between zinc and the muriatic acid. The muriat[illegible] ammoniac is known however to act readily upon copper as well as upon zinc, and yet we find that this ſalt in a ſtate of ſolution powerfully excites the energy of a pile compoſed of zinc and copper, a fact which ſeems to militate againſt our hypotheſis. With a view to illuſtrate this point the following experiments were performed: 1ſt. A piece of zinc was expoſed to the action of a ſaturated ſolution of the muriate of ammoniac, a ſmall quantity of hidrogen gas was diſengaged, the fluid acquired an exceſs of alkali, and after ſome time the metal became covered with a white cruſt. 2. A clean plate of copper was placed horizontally in a ſolution of the muriate of ammoniac; after a few hours the upper ſurface of the metal was covered with a bright green cruſt, while the under ſurface became beautifully ſtudded with ſmall, tranſparent, and nearly colourleſs cryſtals. Theſe cryſtals after expoſure to the air aſſumed a green hue, ſimilar

Probably the diſengagement of electricity is always attended with oxidation in a metal.

The true theory ſuppoſed to be that of Volta combined with that of oxidation.

Exper. I. Zinc expoſed to ſaturated ſolution of muriate of ammonia cauſed emiſſion of hidrogen, abſorbed ſome acid, and became white.

Exp. 2. Copper in like circumſtances was oxided and abſorbed acid, &c.

ſimilar to that of the cruſt on the other ſide of the copper. This green cruſt was ſcarcely acted upon by water, but was rapidly diſſolved by ammoniac, and the ſolution became of a deep blue colour. It ſeemed therefore to be a muriated oxide of copper. The cryſtals were only in part ſoluble in water, the inſoluble part became browniſh; it was quickly diſſolved by ammoniac, and a deep blue was produced. The water in which the cryſtals had been digeſted was without colour or ſmell, but upon the addition of a few drops of cauſtic potaſh, it exhaled a ſtrong odour of ammoniac. Theſe cryſtals therefore appeared to conſiſt of muriate of ammoniac united to a quantity of the oxide, or muriate of copper. The ſides of the glaſs were lined with a coating of the green oxide, and as the ammoniac continued to evaporate, the quantity of this oxide was increaſed. The fluid in which the copper had been digeſted exhibited alkaline properties, and was of a deep blue colour; it contained the ammoniated oxide of copper. 3. A piece of zinc and a piece of copper ſimilar to thoſe employed in the former experiments, were placed in a ſolution of the muriate of ammoniac in contact with each other. The zinc was oxidated as in the former caſe, but the fluid remained colourleſs, and the copper acquired no green cruſt; though after being removed from the liquor and expoſed for ſome time to the atmoſphere, it gradually acquired a thin covering of oxide. The liquor appeared to conſiſt of muriate of ammoniac with a ſmall exceſs of alkali. In this experiment we may conjecture, that the zinc poſſeſſing a ſtronger attraction for oxigen than the copper, abſtracts it from the water, and the hidrogen which is then diſengaged either prevents the oxidation of the copper from taking place, or reduces the oxide as quickly as it is formed. 4. When the zinc and copper were placed in ſeparate glaſſes of the ſolution of the muriate of ammoniac, and the metals were connected by a ſilver wire, the zinc became oxidated as uſual: the copper was in appearance ſcarcely acted upon, but the fluid acquired a blue tinge, which proves that the ammoniated oxide of copper is formed, and conſequently that the muriate of ammoniac and water are decompoſed, and the copper oxidated, though theſe effects take place to a much leſs extent than in the ſecond experiment. The general reſults are favourable to our hypotheſis, as we learn

Exper. 3. Zinc and copper in contact in a ſimilar ſolution; the zinc only was acted on.

Exper. 4. Zinc and copper expoſed as before, but in ſeparate veſſels, and communicating by a ſilver wire, the copper was little affected. Hence a pile of zinc and copper may be conſidered as if one of the metals

only were oxidable by mur. acid.

learn from them that the aɛtion of the muriate of ammoniac upon copper is in a great meaſure ſuſpended when the copper is in contaɛt with zinc.

On the difference of the eleɛt. at the ends of the pile.

3. Before we can arrive at a perfeɛt theory of the galvanic apparatus, it is neceſſary to aſcertain what is the eſſential difference between the condition of the eleɛtric fluid, as it exiſts in the two ends of the pile. It is eſtabliſhed decidedly by the experiments of Van Marum, that the fluid exiſts in the poſitive ſtate at what is called the zinc end, and in the negative at the oppoſite extremity. When the materials are arranged in the order of ſilver, zinc, and card, it is evident that the end from which the eleɛtric fluid is diſcharged in the poſitive ſtate, is the one contiguous to the oxidating ſurface of the zinc. It is not altogether determined in what conſiſts the difference between the poſitive and negative eleɛtricities, but it is generally ſuppoſed to depend upon the fluid being contained in bodies in a greater or leſs quantity than their natural proportion. Let us examine how far this takes place in the pile of Volta. We may conſider each pair of metallic plates with the interpoſed fluid as a complete apparatus for evolving and receiving the eleɛtric matter; and to whatever extent the pile is increaſed, we ſhall have merely a repetition of the ſame aɛtion. Let Z, fig. 3, plate 8, repreſent the plate of zinc, F the interpoſed fluid, and S the plate of ſilver. At the ſurface of the zinc, *c d*, the eleɛtric matter is diſengaged in conſequence of the oxidation of the metal; this diſengaged eleɛtricity is carried acroſs the fluid, and enters the ſurface of the ſilver *e f*, and being then diffuſed through the body of this plate, the whole of the ſilver acquires the poſitive ſtate, and conſequently *i* becomes the poſitive and *k* the negative end of this little apparatus. Does the plate of zinc in this caſe become abſolutely or only relatively negative? It might be ſuppoſed that no other part of the zinc ſhould have its eleɛtricity charged except that which is oxidated; yet from the experiments of Van Marum and others we are led to conclude, that the zinc becomes abſolutely negative through its whole ſubſtance. Dr. Van Marum informs us, that when the wire of a Leyden phial was applied to the negative end of his large pile, the inſide of the jar became charged with negative eleɛtricity; and as there does not appear to have been any connection between the external coating and the other end

of

of the pile, we muſt conclude that the zinc plate becomes abſolutely negative through its whole ſubſtance. If the zinc became negative only in relation to the quantity of electricity accumulated in the ſilver, it would follow that the ſame effects ſhould be produced whether the ſilver was connected with the negative end of the pile, or with any other metallic ſubſtance of equal bulk; but this is directly contrary to fact. Beſides a variety in the quantity or quality of the electricity, another material difference between the two ends of the pile is the direction of the current. While the electric matter is diſengaged from the oxidating ſurface, it ſeems impoſſible to imagine that it can be received again by the ſame ſurface from which it has been diſcharged, and which is continually diſcharging freſh quantities of the fluid. The electricity is therefore neceſſarily diffuſed through the interpoſed liquid, and from this is taken up by the oppoſite metallic ſurface. Upon the uniform direction of its current it is that the action of the apparatus eſſentially depends; and accordingly we find that its effects are ſuſpended when it is altogether ſurrounded with water, or even when the external edges of the plates are moiſtened. It is a very curious circumſtance in the operation of this apparatus, that it does not appear capable of having its power augmented by permitting the electricity to become accumulated in it. The oxidating ſurfaces of the zinc are continually diſcharging a large quantity of electricity, all of which is carried forward to the poſitive extremity, and does not appear to eſcape from it; yet after the two ends of the pile have been brought to an equilibrium by means of an uniting conductor, the energy and intenſity of the apparatus are inſtantaneouſly reſtored to as great a degree as before the diſcharge. Upon the whole it may be inferred, that though the two extremities of the pile differ from each other in the abſolute quantity of electricity which they contain, yet there is reaſon to ſuppoſe that ſome farther alteration is effected, which at preſent we are unable to explain.

Direction of the current.

Limit of electrization.

4. With reſpect to the operation which takes place at the ends of the wires in the interrupted circuit, whatever idea we may adopt of the comparative ſtates of the electric fluid in the two extremities of the pile, we can ſcarcely doubt that it proceeds from the poſitive to the negative end, or from the wire which emits oxigen to that which emits hidrogen. The oxigen which is diſengaged from the poſitive or diſcharging wire, muſt neceſ-

Effects at the ends of the wires.

ſarily

farily either proceed from the wire itfelf, or be difengaged from the fluid in which the wire terminates. If it proceed from the wire itfelf, we may conclude that in whatever fluid the wire is plunged, the oxigen will ftill be emitted, except the fluid fhould itfelf poffefs an attraction for oxigen. Upon the other fuppofition, however, that the oxigen proceeds from the decompofition of the fluid, in confequence of the electricity uniting itfelf to hidrogen in its paffage between the wires, we may expect to be able to fufpend the production of oxigen, by prefenting to the wire a liquid which contains hidrogen united to fome other element. The liquid cauftic ammoniac promifes to anfwer all the requifite purpofes, and we fhall find that the experiments made upon it by Mr. Cruickfhank and Mr. Davy, though not precifely fimilar in their refults, afford conclufions not unfavourable to our hypothefis. Mr. Cruickfhank employed wires of platina, and when thefe were connected with the pile, and permitted to terminate in ammoniac, he found that the gafes evolved were azote and hidrogen, with fo fmall a mixture of oxigen as to render it probable that this latter fubftance proceeded rather from fome accidental circumftance, than from the effential nature of the operation. Mr. Davy ufed gold wires which terminated in two glaffes of ammoniac, connected to each other by means of mufcular fibres. He found that the pofitive wire evolved gas only in fmall quantity, and that it was a mixture of three parts of oxigen and two of azote, while the other wire gave out hidrogen in confiderable quantity; the pofitive wire was vifibly corroded. I was for fome time at a lofs to reconcile the refults of thefe able experimenters, but upon reflection I was led to conclude that the variation might be produced in part from the nature of the wires which they ufed, and partly from a difference in the energy of their refpective piles. In order that the experiment fhould be unexceptionable, it is neceffary that a metallic conductor be employed which poffeffes little attraction for any of the elements compofing the fluid in which it terminates; as by this precaution any change of compofition which takes place muft depend altogether upon the affinities of the electric fluid. Gold is on this account lefs proper than platina. It may be conjectured that in Mr. Davy's experiment with gold wires, the predifpofing affinity of the oxide of this metal for an oxigenated nitric acid, would caufe the decompofition of a portion both of the

Immerfed in ammonia.

Azote and hidrogen evolved by platina wires in ammonia.

Gold wires;

corrofion;

the water and the ammoniac, and that the oxigen and azote thus liberated, would unite together in ſuch proportion as to generate a ſolvent for the metal. We ſuppoſe the gas which is diſengaged from the poſitive wire to be produced by the decompoſition of the fluid in which it terminates, in conſequence of the ſtrong attraction which ſubſiſts between the electric matter and hidrogen. It may reaſonably be inferred that there is a point of reciprocal ſaturation between theſe ſubſtances, or that a given quantity of electricity cannot paſs between the wires unleſs it be united to a definite quantity of hidrogen. If the electric matter in the poſitive wire exiſt there only in a ſmall quantity, it acquires for itſelf a ſufficient proportion of hidrogen by decompoſing the ammoniac alone; but when the apparatus is large and operates with energy, the ammoniac is inadequate to afford the neceſſary ſupply of hidrogen, and the water becomes alſo decompoſed. I endeavoured to ſubmit the truth of this conjecture to the teſt of experiment, for which purpoſe gold wires were employed terminating in ammoniac, and connected in different inſtances with a greater or leſs number of metallic plates. The reſults were upon the whole favourable, but on account of the minute quantities of gas procured, I was unable to aſcertain its nature with as much accuracy as was deſirable. When the proceſs went on rapidly the water was decompoſed as well as the ammoniac, and if it was continued for a great length of time, the ammoniac attracted carbonic acid gas from the atmoſphere, and thus produced a ſource of error in the reſults. Several other metallic bodies which were interpoſed between the ends of the pile, were all of them more or leſs corroded in conſequence of their ſtrong attraction for oxigen. When copper and tin were uſed, the oxides were diſſolved by the ammoniac; in the former caſe this was rendered viſible by the blue colour which the liquor aſſumed, and in the latter by the precipitation of the oxide of tin effected by the addition of ſulphuric acid. The ſlips of tin foil which were employed had their extremities changed to a brown colour, particularly that which was connected with the negative end of the apparatus.

The ſolution of ammonia has its alkali decompoſed by a weaker power, and alſo water by a ſtronger.

Experiment.

After theſe obſervations upon the action of the poſitive wire, there remains little to be added on the ſubject of the oppoſite one; it merely receives the electric fluid after its paſſage through

The minus wire of the pile.

through the water, and tranfmits it to the negative end of the pile. If the wire terminate in pure water, the hidrogen is difengaged in the form of gas; but if a metallic falt be diffolved in the water, the hidrogen is attracted by the oxide, and this laft reduced to the ftate of a metal. The phenomena exhibited in this procefs are often peculiarly beautiful, and the extreme delicacy which the apparatus poffeffes in difcovering even the minuteft metallic impregnations, fuggefts its probable utility in the analytical operations of chemiftry.

How far galvanifm may modify our electric theories.

5. A very interefting fubject of inquiry here prefents itfelf, viz. in what degree will our theoretical notions of the action and properties of the electric fluid be influenced by the galvanic difcoveries? The theory of Dr. Franklin, as well as the elegant one propofed by M. AEpinus, feems to be adapted only to thofe cafes in which the electric fluid previoufly exifting in bodies, was altered either with refpect to its difpofition or quantity. But we are now in poffeffion of an apparatus by which it is actually generated, a circumftance with which we were until lately unacquainted, and confequently our hypothefis, being a general deduction from facts, will at leaft require to be extended, perhaps in fome inftances new-modelled.

Chemical theory is alfo much benefited by thefe difcoveries.

The pile of Volta affords many illuftrations of the modern chemical doctrines, which appear the more ftriking as effected by experiments made with a different object in view, and produced by an agent which cannot be fuppofed capable of affecting the refults. The grand queftion refpecting the decompofition of water, if it ftood in need of farther fupport, derives powerful confirmation from the experiments firft performed by Mr. Nicholfon and Mr. Carlifle, and fince repeated with various modifications. The experiments made with the galvanic apparatus alfo confirm our ideas refpecting the compofition of ammoniac and nitric acid, and confiderably add to our knowledge of the action which takes place between acids and metals. They beautifully exhibit the powerful effects produced by oxigen and hidrogen when in a condenfed or uncombined ftate, and promife to afford the chemift a valuable opportunity of employing in his different operations thefe bodies in a ftate of abfolute purity. Every one interefted in the progrefs of philofophical knowledge, will regard with admiration the difcovery of an apparatus, which has enabled him to develope fome of

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the moſt hidden operations of nature, and will conſider the name of Volta as worthy to be claſſed with thoſe of Lavoiſier, Prieſtley, Cavendiſh, and the other illuſtrious founders of the pneumatic chemiſtry.

III.

A Summary of the moſt uſeful Parts of Hydraulics, chiefly extracted and abridged from Eytelwein's Handbuch der Mechanik und der Hydraulik. Berlin, 1801. *By* THOMAS YOUNG, *M. D. F. R. S.*

(*Concluded from Page* 35.)

CHAPTER 9. Of the motion of water in pipes.

Motion of water in pipes.

The author has attempted to ſimplify this ſubject nearly in the ſame manner as that of the motion of rivers, and apparently with conſiderable ſucceſs. He obſerves, that the head of water may be divided into two parts, one of which is employed in producing velocity, the other in overcoming the friction: that the height employed in overcoming the friction muſt be as the length of the pipe directly; and alſo directly as the circumference of the ſection, or as the diameter of the pipe, and inverſely as the content of the ſection, or as the ſquare of the diameter; that is, on the whole, inverſely as the diameter; this height too muſt vary, like the friction, as the ſquare of the velocity.

To find the velocity;

Hence $f=\frac{al}{d}.v^2$, f denoting the height due to the friction, and a a conſtant quantity: therefore, $v^2=\frac{fd}{al}$. Now the height employed on the friction, correſponds to the difference between the actual velocity and the actual height, or $f=h-\frac{v^2}{b^2}$, where b is the coefficient for determining the velocity from the height; conſequently, $v^2=\frac{b^2dh-dv^2}{ab^2l}$, and $v^2=\frac{b^2dh}{ab^2l+d}$. Now $b=6.6$, and from Buat's experiments, ab^2 is determined to be .0211; which agrees the moſt accurately where the velocity is between 6 and 24 inches in a ſecond. Whence we have

$v^2=$

$v^2 = \frac{43.6dh}{.0211l+d}$, or $v = 45.5\sqrt{\left(\frac{dh}{l+47d}\right)}$; but it is fomewhat more accurate to make $v = 50\sqrt{\left(\frac{dh}{l+50d}\right)}$; all the meafures being expreffed in Englifh feet.

allowance for flexure.

When the pipe is bent into angles, or rather arcs, we muft diminifh the velocity thus found, by taking the product of its fquare multiplied by the fum of the fines of the feveral angles of inflection, and then by .0038; which will give the degree of preffure employed in overcoming the refiftance occafioned by the angles: and deducting this height from the height correfponding to the velocity, we may thence find the corrected velocity.

Compound pipes.

Mr. Eytelwein proceeds to inveftigate, both theoretically and experimentally, the difcharge of water by compound pipes, with apertures of various dimenfions between them: he allows at each orifice for the contraction of the ftream, and calculates the height neceffary to produce the increafe of velocity in each inftance, allowing alfo for the friction of the pipe. But the velocity thus found is fomewhat fmaller than the refult of his experiments; probably becaufe the whole of the force of the water accelerated at any orifice, is not immediately loft as foon as it arrives at a wider part of the pipe. The afcent of water in a compound pipe, to the level of a refervoir, is next confidered, a cafe which often occurs in pump-work; and an approximation to the velocity of afcent is deduced from theory and compared with experiment.

Jets of water.

Chapter 10. Of jets of water.

This chapter contains little that is new or interefting; it is well known, that the velocity of a jet is greateft when it fprings through an orifice in a thin plate, and in this cafe, the height falls little fhort of that of the refervoir.

Impulfe or hydraulic preffure of water.

Chapter 11. Of the impulfe or hydraulic preffure of water.

There are three principal cafes of the impulfe of water falling perpendicularly on plane furfaces: when a detached jet of water ftrikes the plane; when the plane moves in an unlimited extent of water, or is very fmall in refpect to a ftream that ftrikes it; and when the impulfe takes place in a limited channel.

When the ftream lofes all its velocity;

Suppofing a ftream of water to ftrike againft a plane fo as to lofe all its motion, it is obvious, that the force that deftroys the

the motion muſt be equal to the force that generates it; that is, to the weight of the column of water operating during the time neceſſary for its acquiring the given velocity: and the quantity of water arriving during this time, being equal to twice the column of which the length is the height due to the velocity, the hydraulic preſſure muſt be twice the weight of ſuch a column. The relative impulſe againſt a plane in motion, muſt be determined from the difference of the velocities: but when all the water of a ſtream ſtrikes againſt a plane, the effect of the impulſe may be more ſimply determined, as if a ſolid body ſtruck the plane with the relative velocity; and this is nearly what happens in underſhot water-wheels. or when the plane recedes.

When a detached jet ſtrikes againſt a plane, it appears, from the experiments of Boſſut and Langſdorf, that its effect is equal to the weight of an equal column of twice the height due to the velocity; but the plane muſt be at leaſt four times as large in diameter as the jet; if it be only of the ſame ſize, the effect will be but one half as great. In an unlimited ſtream, the impulſe is alſo nearly determined from the height correſponding to the velocity; and it appears, that the effect is nearly doubled by confining the ſtream to prevent its diverting laterally from the float-boards. Detached jet. Unlimited ſtream; confined in a channel.

For oblique ſurfaces, the effect of a detached jet in its own direction, appears to vary as the ſquare of the ſine of the angle of incidence; but, for motions in open water, we muſt add to this ſquare about $\frac{2}{5}$ of the difference of the ſine from the radius: a correction which is tolerably accurate, until the inclination becomes very great. Mr. Eytelwein found the reſiſtance to the motion of a ſphere nearly $\frac{4}{5}$ of the reſiſtance to a circle equal to its ſection: perhaps it was a hemiſphere, otherwiſe it is difficult to reconcile the reſult with other experiments in which it has appeared to be only $\frac{2}{5}$. Oblique ſurfaces oppoſed to a jet.

Mr. Eytelwein informs us, that at the temperature 14° of Reaumur, or $63\frac{1}{2}$ of Fahrenheit, a cubic foot of diſtilled water weighs 66.0656 pounds of Cologne, or 65.9368 commercial pounds of Berlin. According to Sir George Shuckburgh's experiment, an Engliſh cubic foot of diſtilled water at 66° weighs 997 ounces avoirdupois; and water expands for every degree .000165: hence the pound of Cologne is 1.0312 Engliſh avoirdupois pounds, and that of Berlin 1.0332. Weight of diſtilled water.

Chapter 12. Of overſhot water-wheels. Overſhot wheels;

The power which operates upon overſhot wheels, is divided

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into two parts, one derived from the weight of the water in the cells or buckets, the other from the impulſe of the water falling on it: the effect of the firſt is conſtant, that of the ſecond varies with the velocity: the maximum is found to be when the velocity is half that of the water received; but the variable part being the ſmaller, the rule is of little practical conſequence, and the velocity of the wheel is generally greater than this.

advantage of reverſing their rotation.

The author obſerves, that by turning the ſtream back upon the nearer half of the wheel, we remove the reſiſtance of the lower water, ſince it runs off in the ſame direction with that of the water-wheel.

Underſhot wheels.

Chapter 13. Of underſhot water-wheels.

Floatboards ſhould riſe perpendicularly.

The author enters into a minute deſcription of the parts of an underſhot water-wheel: he obſerves, that the moſt advantageous poſition for the floatboards in a ſtraight channel is, when they are perpendicular to the water at the time that they riſe out of it: that only one half of each ſhould ever be below the ſurface, and that from three to five ſhould be immerſed at once, according to the magnitude of the wheel. When there is ſufficient fall, the floatboards ſhould be divided and made into buckets, ſo that the wheel may become a breaſt wheel; the poſition of the external portion being ſuch, that a line drawn through it at the time when the water enters, may divide the vertical radius in the ſame proportion that it divides the quadrant of the circumference; that is, if the water is received, for inſtance, at one third of the quadrant from the bottom, the line muſt leave one third of the radius above it. A formula is laid down for calculating the actual force of a given ſtream of water on a wheel, and it is ſhown, that half the velocity of the ſtream is that which gives the maximum of effect, the theory agreeing perfectly with the experiments of Smeaton and others: for, ſince the effect is eſtimated by the product of the force into the velocity of the parts upon which it acts, and ſince the force is in this caſe ſimply as the relative velocity, becauſe the quantity of water is given, and the whole of it is ſuppoſed in all caſes to act; therefore, the effect will be expreſſed by the product of the relative and abſolute velocity of the wheel, or $e=ra$; but $r=v-a$, v being the velocity of the ſtream, and $ra=av-aa$, which is obviouſly greateſt when $v=2a$, as is evident either by taking the fluxion, or by conſidering that the greateſt ordinate of a ſemicircle is the radius.

Underſhot wheel ought to work with half the velocity of the ſtream.

To

To ſhow the advantage of breaſt wheels over common underſhot wheels, the author quotes Mr. Banks's experiments. He alſo obſerves, that by placing two wheels after each other in the ſame ſtream, about one fourth more force may be obtained than either by a ſingle wheel, or by two wheels ſide by ſide; but that a ſingle wheel has leſs friction, and is generally leſs expenſive.

Breaſt wheels much better than underſhot.

Chapter 14. Of the properties of the air, as far as they relate to hydraulic machines.

What Mr. Eytelwein quotes as Mariotte's diſcovery of the increaſe of the air's denſity in proportion to the preſſure, was well known to Hooke and Boyle. From the experiments of Woltmann and Schober, he remarks, that the force of the wind againſt a perpendicular plane, is nearly equal to four thirds of the weight of a column of air, of a length equal to the height due to the velocity. The height of a column of water nearly equivalent to the force or reſiſtance may be found, by taking the ſquare of $\frac{1}{200}$ of the velocity in a ſecond, in Engliſh feet.

Force of air againſt a plane;

Rule for finding it.

Thus, if the velocity were 1000 feet in a ſecond, the reſiſtance would be equal to a column of water in the ſame ſurface, 25 feet in height; and the reſiſtance to a ſphere about half as much.

Example.

For another example, if we had a cubic foot of a ſubſtance equal in ſpecific gravity to water, and were deſirous of knowing the greateſt velocity that it could acquire by falling through the air; the height of the column of water is here 1, and its ſquare root 1, which multiplied by 200 gives 200 feet in a ſecond for the velocity, when the reſiſtance would be equal to the weight, which of courſe is the limit beyond which the velocity could never paſs. Hence we may form an idea of the utmoſt velocity that a ſtone, of moderate ſize, could acquire in deſcending from the upper regions of the atmoſphere, or even from the neighbourhood of the moon; a velocity that would be much leſs than that of a bomb or a cannon ball, even when it may be followed by the eye.

Inſtance of the utmoſt velocity poſſible to be had by a body falling in the air:

much leſs than that of a cannon ball.

Again, Mr. Garnerin's parachute contains about 860 ſquare feet of ſurface, and weighs, together with the aeronaut ſuſpended from it, about 230 pounds. Here the weight is $\frac{23}{86}$ of a pound for each ſquare foot, which is equivalent to $\frac{1}{230}$ of a foot of water; multiplying the ſquare root by 200, we have about 13 feet in a ſecond for the utmoſt velocity; which is the

Parachute of Garnerin.

fame as if one leaped from a height of between two and three feet. Mr. Garnerin, however, finds the mean velocity of defcent only eight feet, which agrees better with the experiments of Borda, in which the refiftance appeared to be $\frac{5}{3}$ of the weight of the column due to the velocity, and exceeded this proportion as the furface increafed in magnitude.

Syphons.

Chapter 15. Of fiphons.

For eftimating the difcharge of a fiphon, the head of water muft be reckoned equal to the difference between the levels of the furface of the water, and of the lower orifice. The author obferves, that the theory of waves has been treated in a new and improved manner by Lagrange in his Mécanique Analytique. The problem is, however, not yet completely folved: Lagrange's formula includes the depth of the water agitated as a given quantity, but it does not inform us how to determine this depth from theory.

Sucking pumps.

Chapter 16. Of fucking pumps.

The length of a fucking pump muft never be greater than 30 feet below the moveable valve: and there may be a lofs of time in the afcent of the water, unlefs it be made even a few feet fhorter. The motions to be produced, and the refiftances to be overcome, are confidered in detail: but the author refers, for ftill further information, to Langfdorf's Treatife on Machinery.

Nature of the ftroke.

The velocity of the ftroke fhould never be lefs than four inches, nor greater than two or three feet in a fecond; the ftroke fhould be as long as poffible, to prevent lofs of water by the frequent alternations of the valves. The diameter of the pipe fhould be about $\frac{2}{3}$ or $\frac{3}{4}$ of that of the barrel. The lifting pump is alfo here defcribed; it only differs from the fucking pump in having the lower valve moveable, and the upper one fixed. A number of valves and piftons are defcribed in this chapter; chiefly from models of Englifh manufactory.

Forcing pumps;

Chapter 17. Of forcing pumps.

In defcribing the different kinds of folid pifton, the author gives the preference to that which has a conical leather projecting on each fide; but remarks, that there is another form which has the advantage in the fituation of the ring for receiving the rod, which is precifely in the centre of the pifton, and is therefore fitter for communicating motion in each direction. He fays, that where the barrel is well polifhed, the pifton may be ufed without either wadding or leather.

made without leathering.

The

The firſt pump, invented above a century before Chriſt, by Cteſibius of Alexandria, to whom alſo muſic is indebted for the organ, and whoſe name Mr. Eytelwein mentions in ſpeaking of ſucking pumps, was in reality a forcing pump, as may eaſily be collected from its deſcription by Vitruvius (L. X. cap. 12.) The firſt pump was a forcer

Chapter 18. Of mixed pumps, or the combination of ſucking and forcing pumps. Mixed pumps.

When the lower valve is above the ſurface of the water, the forcing pump can only raiſe the water by ſuction, but the conſtruction remains the ſame. Such is Mr. Buchanan's patent ſhip pump. De la Hire's pump is more complicated; both the aſcending and deſcending ſtrokes of the piſton being made effective, by means of a double apparatus of valves and pipes.

Chapter 19. Of acting columns of water. Included columns acting by preſſure.

The mechaniſm of a pump may be employed for converting the weight of water deſcending in its barrel, to the purpoſe of working another pump. The author deſcribes a machine of this kind invented by Mr. Höll, and improved by Langſdorf. A ſimilar arrangement, uſed in Cornwall, has lately been deſcribed in Nicholſon's Journal, by Mr. Trevithick. The only objection to it appears to be the magnitude of the friction.

Chapter 20. Of the ſpiral pump. The ſpiral pump or horizontal helix forcing water up a pipe.

If we wind a pipe round a cylinder, of which the axis is horizontal, and connect one end with a vertical tube, while the other is at liberty to turn round and receive water and air in each revolution, the machine is called a ſpiral pump. It was invented about 1746, by Andrew Wirz, a pewterer in Zurich, and was employed at Florence with Bernoulli's improvement, in 1779. At Archangelſky, near Moſcow, a pump of this kind was erected in 1784, which raiſed a hogſhead of water in a minute, to a height of 74 feet, and through a pipe 760 feet in length. The force employed is not mentioned, we may therefore conjecture that it was turned by water. Mr. Eytelwein enters very minutely into calculations of the effect of ſuch a machine under different circumſtances; and the reſults of theory, as well as of experiment, are ſuch, as to induce us to expect that it will in time come into common uſe, inſtead of forcing pumps of a more complicated and expenſive conſtruction. The water-tight joint preſents the only difficulty: the pipe

pipe may form either a cylindrical, a conical, or a plane ſpiral, and it appears to be uncertain which is the moſt advantageous: the vertical pipe ſhould be nearly of the ſame dimenſions as the ſpiral pipe, which may without difficulty be made of wood.

Screw of Archimedes.

Chapter 21. Of the ſcrew of Archimedes, or the waterſnail, and of the waterſcrew.

The ſcrew of Archimedes conſiſts either of a pipe wound ſpirally round a cylinder, or of one or more ſpiral excavations, formed by means of ſpiral projections from an internal cylinder, covered by an external coating ſo as to be water-tight. But if the coating is detached, ſo as to remain at reſt while the ſpirals revolve, the machine is called a waterſcrew. Mr. Eytelwein enters into an accurate determination of the effects of theſe machines in given circumſtances, and the reſults of the theory agree remarkably well with experiment. He obſerves, that the ſcrew of Archimedes ſhould always be ſo placed, as to fill exactly one half of a convolution in each turn; and that very unfavourable reports have ſometimes been made of the machine from want of attention to this circumſtance; for when the orifice remains conſtantly immerſed, the effect is very much diminiſhed: this appears alſo to have happened in ſome late experiments in London. Where the height of the water is ſo variable as to render this precaution impoſſible, Mr. Eytelwein prefers the waterſcrew; although, in this inſtrument, one third of the water generally runs back, and it is eaſily clogged by accidental impurities in the water. The ſcrew of Archimedes is generally placed ſo as to form an angle of between 45° and 60° with the horizon; but the open waterſcrew at an angle of 30° only: for great heights, the ſpiral pump is preferable to either.

The water-ſcrew.

Caution for placing the ſcrew of Archimedes.

Bucket wheels.

Chapter 22. Of bucket wheels and throwing wheels.

In the conſtruction of wheels for raiſing water in buckets, there is little room for refined theory; whether the buckets be fixed or ſuſpended on an axis. It is ſometimes convenient to raiſe water to the height of 3 or 4 feet by the revolution of a wheel with ſimple floatboards; and ſuch a wheel may be either in a vertical or an inclined poſition; it muſt of courſe be incloſed in a ſweep.

Throwing wheel.

Chapter 23. Of cellular pumps and paternoſter works.

Water has been fometimes raifed by ftuffed cufhions connected with an endlefs rope, and caufed by means of two wheels or drums to rife in fucceffion in the fame barrel, and to carry water with them: but the magnitude of the friction appears to be an objection. From the refemblance of the apparatus to a ftring of beads, it has been called a paternofter work. When flat boards are united by chains, and employed inftead of thefe cufhions, the machine may, without impropriety, be called a cellular pump: here the barrel is generally fquare, and placed in an inclined pofition. But thefe machines are very rarely employed.

Cellular pumps or paternofter works.

The chain-pump.

Chapter 24. Of inftruments for meafuring the velocity of ftreams of water.

The fuperficial velocity of a ftream is afcertained without difficulty, by obferving in calm weather the motion of a body barely floating on it. But it is more difficult to determine the velocity of a river at a confiderable depth. Pitot's tube, as improved by Buat, furnifhes one of the eafieft methods. A funnel is prefented to the ftream, and the water in a vertical tube connected with it, is elevated above the level of the river, nearly to the height correfponding to the velocity: but the refult will be more accurate, if the funnel be covered by a plate perforated only in the centre by a fmall orifice: in this cafe, the elevation in the tube will be half as great again as the height due to the velocity. Other inftruments for appreciating the impulfe of the water againft a flat board, require fome previous comparative obfervation. In Woltmann's hydrometrical fly, the number of revolutions of a wheel, in a given time, indicates the velocity of the water, which ftrikes againft two inclined planes, and carries round the arm to which they are fixed.

Inftruments for meafuring the velocity of ftreams of water.

It is prefumed, that this abridged account of Mr. Eytelwein's book, will not only do juftice to his diligence and ingenuity, but will convey to the Englifh reader fome matter perfectly new, and capable of frequent application in practical hydraulics; which is perhaps of the more value, as there is little probability that the work will be tranflated at length. To diffeminate information of this kind muft always be the principal object of the Journals of the Royal Inftitution.

Concluding addrefs.

Y.

IV. *Experiments*

IV.

Experiments and Obſervations on certain ſtony and metalline Subſtances, which at different Times are ſaid to have fallen on the Earth; alſo on various Kinds of native Iron. By EDWARD HOWARD, *Eſq. F. R. S. From the Philoſophical Tranſactions,* 1802.

(Continued from Page 263, Vol. II.)

Chemical examination of ſtones fallen on the earth.

I PROCEED to conſider the aſſiſtance to be derived from chemiſtry in diſtinguiſhing theſe ſtones from all other known ſubſtances, and in eſtabliſhing the aſſertion, that they have fallen on the earth.

Objections to the analyſis of the ſtone preſented by the Abbé Bachelay,

The analyſis made by the French academicians of the ſtone preſented to them by the Abbé Bachelay, was in part conducted by the ever to be deplored Lavoiſier; but it was performed before that celebrated author had enriched chemiſtry with his laſt diſcoveries, and before he had given birth to the ſyſtem under which it flouriſhes. The reſult of this analyſis might well induce the concluſion, that the ſubject of it was common pyritical matter. It was unfortunately made of an aggregate portion of the ſtone, and not of each diſtinct ſubſtance irregularly diſſeminated through it. The proportions obtained were, conſequently, as accidental as the arrangement of every ſubſtance in the maſs.

and to that of M. Barthold.

The analyſis of M. Barthold, of the ſtone of Enſiſheim, is ſubject to the ſame objections: but, after having the advantage of the foregoing deſcriptions, the reſearches which follow cannot be ſuppoſed altogether liable to a ſimilar fatality.

EXAMINATION OF THE STONE FROM BENARES.

Chemical examination of the ſtone from Benares.

This ſtone, as the Count de Bournon has already remarked, has the moſt diſtinguiſhed characters. Indeed it is the only one of the four ſufficiently perfect (if I be allowed that expreſſion) to be ſubjected to any thing approaching to a regular analyſis.

The black coating deprived of magnetic parts

The cruſt, or external black covering, is the firſt ſubſtance to which the attention is naturally directed. When a portion of

of this cruſt had been detached with a knife or a file, and finely pulverized, I ſeparated the particles attractable by a magnet, and digeſted the unattractable portion with nitric acid, which was preſently decompoſed; but, owing to a ſtrong adherence of ſome of the interior and earthy parts of the ſtone, it did not diſentangle the coating or metalline part without ſome difficulty. The acid being ſufficiently neutralized, the ſolution was paſſed through a filter, and ſaturated to exceſs with ammonia. An abundant precipitate of oxide of iron was produced; and, when this oxide was ſeparated, I obſerved the ſaline liquor to have a greeniſh colour. I evaporated it to dryneſs, and rediſſolved the dry ſalt in diſtilled water. No precipitate was formed during the evaporation, nor was the colour of the ſolution entirely deſtroyed. It appeared to me like a triple ſalt, deſcribed by Mr. Hermſtadt * as an ammoniacal nitrate of nickel. By examination with pruſſiate of ammonia, it yielded a whitiſh precipitate, inclining to a violet colour; and, by various properties, I was ſoon confirmed in the opinion, that nickel was preſent. Since I ſhall have occaſion more than once to treat of the triple compound, and ſince it has been only mentioned by Mr. Hermſtadt, it is neceſſary now to detail ſome of its diſtinctive characters. The ſame chemiſt informs us, that the three mineral acids, with ammonia, enter into ſimilar combinations with nickel; and I have obſerved, that oxide of nickel can be diſſolved by nitrate and muriate of ammonia. The muriate ſeems to take up the largeſt quantity. The colour of this ſalt is by no means uniform: it is ſometimes graſs green, violet, roſe colour, inclining to purple, and I have ſeen it almoſt colourleſs. It ſeems to be purple, and to incline to roſe colour and violet, when all the oxide of nickel is not united to both acid and alkali, but, from the deficiency of ſalt, is held in ſolution by an exceſs of ammonia. In this caſe, evaporation, of courſe, precipitates the nickel in the ſtate of oxide, which is of a whitiſh green colour.

was digeſted in nitric acid; which it decompoſed.

The iron ſolution was precipitated by ammonia.

Ammoniacal nitrate of nicke remained.

Diſtinctive characters of this triple ſalt.

The nickel cannot be *precipitated* from a perfectly formed triple ſalt, by any reagent I have tried, except by a pruſſiate, or a hidrogenized ſulphuret of ammonia. Potaſh and lime, as

The nickel is precipitable by a pruſſiate or an hidrogenized ſulphuret of ammonia.

* Annales de Chimie, Tom. XXII. p. 108.

well

well as, I presume, other bodies, standing in the order of affinities before ammonia, decompose the salt; but the nickel is then continued in solution by the disengaged ammonia.

The liquid was always examined to detect copper if it had been present.

As it may be imagined that I have occasionally met with copper, when I describe a violet or purple ammoniacal solution, it is right to observe, that to avoid this error, I have either reduced the liquor to a neutral state, and endeavoured, without success, to obtain from it a precipitate, with a solution of sulphureted hidrogen gas; or, by adding an acid to slight excess, and immersing a piece of iron, I have not been able to detect a trace of copper. These, and many other trials, when they do not appear to be made before the estimation of the quantities of nickel, have been constantly made afterwards.

The coating contained iron nearly metallic, and some nickel.

But, to return to the incrustation or coating of the stone, the decomposition of the nitric acid shewed the presence of matter at least nearly metallic, although not attractable; and the examinations made of the liquor, from which the iron was precipitated, ascertained the presence of nickel beyond dispute. The difficulty of obtaining the coating of the stone, either distinct from matter not belonging to it, or in sufficient quantity, induced me to relinquish the idea of attempting to give the proportions of its constituent parts.

The shining particles of the stone

The stone being deprived of its covering, the shining particles irregularly disseminated, next demand examination. I first examined the pyrites. Their very loose texture made it exceedingly difficult to collect the weight of 16 grains, which was however effected by the dexterity of the Count de Bournon.

were digested in dilute muriatic acid.

I digested these, at a low heat, with weak muriatic acid; which acted gradually, and disengaged a trifling but sensible quantity of sulphureted hidrogen gas. After several hours, I found the acid discontinued its action. The whole metalline part appeared in solution; but sulphur and earthy particles were observable. The sulphur, from its small specific gravity, was suspended through the solution; whilst the earthy matter, which could not be separated by mechanical means, was fortunately left at the bottom of the digesting vessel. I decanted off the solution, holding suspended the sulphur; and, by repeated washing, separated every thing belonging to the pyrites

It dissolved the metal, and suspended the sulphur, and the earthy portion subsided.

rites

rites from the insoluble earthy matter, the subtraction of which reduced the weight of real pyrites to 14 grains. I next obtained the sulphur, by filtration. When it was as dry as I could make it, without fear of its being sublimed, its weight was two grains. To the filtrated liquor I added nitrate of barytes, by way of detecting any sulphuric acid which might have been present; but no cloudiness ensued. I then separated, by sulphate of ammonia, the barytes thus added, and precipitated the iron with ammonia. The liquor, on the subsidence of oxide of iron, appeared of a violet purple colour: it contained nickel, which I threw down with sulphureted hidrogen gas, there being already a sufficient excess of ammonia in the saline liquor to form an alkaline hidrogenized sulphuret. The oxide of iron, after ignition, weighed 15 grains; and the sulphuret of nickel, reduced to an oxide, weighed, after the same treatment, something more than one grain. The proportions of the substances contained in the pyrites of the stone from Benares, may therefore be considered nearly thus.

The metals were iron and some nickel;

	Grains.
Sulphur - - - - - -	2
Iron - - - - - - -	$10\frac{1}{2}$
Since 15 grains of the oxide represent about that quantity of iron,	
Nickel, nearly - - - - - -	1
Extraneous earthy matter - - -	2
	$15\frac{1}{2}$

Component parts, sulphur, much iron, nickel, and earth.

It is observable that, notwithstanding the loss appears to be only half a grain, it was probably more, because the sulphur could not be reduced to the same state of dryness in which it existed when in combination with the iron; not to say that it was, in a small degree, volatilized with the hidrogen gas disengaged during the solution.

Remarks.

The weight of nickel is a mere estimation. We are not yet sufficiently acquainted with that metal to speak of it with accuracy, except as to its presence. Upon the whole, however, it may be concluded, that these pyrites are of a very particular nature; for, although Henkel has observed that sulphur may be separated from pyrites by muriatic acid, it is by no means the usual habitude of pyrites to be of such easy decomposition.

The

The other ſhining particles being malleable iron,

The other ſhining particles immediately ſeen, when the internal ſtructure of the ſtone is expoſed, are the malleable iron. Before I ſtate the examination of this iron, I muſt remark, that preliminary experiments having ſhewn me it contained nickel, I treated ſeveral kinds of the moſt pure irons I could obtain, with nitric acid; and precipitated the oxide from the metallic ſalt by ammonia. The quantity of oxide I obtained from 100 grains of iron, was from 144 to 146. I may conſequently infer, that 100 grains of pure iron acquires, by ſuch a proceſs, 45 grains of oxigen; and that, whenever a metallic ſubſtance, ſuppoſed to be iron, does not, under the ſame circumſtances, acquire the ſame proportionate weight, ſomething is either volatilized, or left in ſolution. Hence, when a metallic alloy of nickel and iron preſents itſelf, a judgment may, at leaſt, be formed of the quantity of nickel, by the deficiency of weight in the precipitated oxide of iron.

were diſſolved in nitric acid, and precipitated by ammonia,

This mode of treatment was not allowed me in the examination of the coating of the ſtone, becauſe it was impoſſible to know in what ſtate of oxidizement the iron exiſted. But, as the particles diſſeminated through the whole maſs, are clearly metallic, a very tolerable idea of the quantities of nickel contained in them will be obtained, by noting the quantity of oxide of iron ſeparated, as above deſcribed. 25 grains of theſe metallic particles were therefore heated with a quantity of nitric acid, much more than ſufficient to diſſolve the whole. Some earthy matter, which, as in a former caſe, was not ſeparable by mechanical means, remained after a complete ſolution of the metal had been effected. This earthy matter, after being ignited, weighed two grains. The real matter of the preſent examination, was therefore reduced to 23 grains, and was in complete ſolution. I added ammonia to a very ſenſible exceſs. The oxide of iron was thereby precipitated, and, being collected and ignited, it weighed 24 grains; whereas, according to my experiments, 33½ grains ſhould have been produced from the ſolution, had it contained nothing but iron.

which left nickel in triple ſolution of which the quantity was eſtimated.

I examined the ſaline liquor, when free from ferruginous particles, and diſcovered it to be the triple ſalt of nickel. Hence, allowing for loſs, the quantity of nickel may be eſtimated, by calculating the quantity of iron contained in 24 grains of oxide. Thus, if 145 grains of oxide contain 100 of iron, about 16½ are contained in 24 of oxide. This would ſuppoſe the 23

grain

grains of alloy to consist of $16\frac{1}{2}$ iron and $6\frac{1}{2}$ nickel; which, if the usual loss be added to the $16\frac{1}{2}$ grains of iron, and deducted from the nickel, may not be very remote from the truth.

I shall next examine the globular bodies, also irregularly dispersed throughout the stone. A number of them were reduced to fine powder; but nothing metallic could be separated by the magnet. As a preliminary experiment, I sought for pyrites, by digestion with muriatic acid; but no hepatic smell was in the least perceivable, nor was white carbonate of lead at all altered by being held over the mixture. I therefore conclude these globular bodies do not envelope either iron or pyrites. By way of analysis, I treated 100 grains with potash, in a silver crucible; and, after the usual application of a red heat, separated as much silica as possible, by muriatic acid and evaporation. The silica being collected on a filtre, carbonate of potash was added to the filtrated liquor; by which, a precipitate, almost wholly ferruginous, was produced. This precipitate was collected in the common way; then boiled with potash, to extract alumina; and, by supersaturating the alkaline liquor with muriatic acid, and precipitating by carbonate of ammonia, an earth was gathered, which I afterwards found to be partly, if not intirely, siliceous. After redissolving, in muriatic acid, the portion of the ferruginous matter rejected by the potash, I precipitated by ammonia, what I took to be intirely oxide of iron; but, after igniting it, and again attempting to redissolve the whole in muriatic acid, more silica was left. The non-existence of lime was proved, by the addition of carbonate of ammonia, immediately after the same alkali, pure, had thrown down what I took wholly for oxide of iron. I had now obtained every thing in the subject of my analysis, except magnesia and nickel. The former, and a trace of the latter, were held by carbonic acid in the liquor, from which the ferruginous precipitate was, in the first instance, thrown down by carbonate of potash; and the latter was found in the last named muriate of ammonia. I disengaged the magnesia, by the assistance of potash, and by evaporating to dryness. The oxide of nickel was precipitated by hidrogenized sulphuret of ammonia.

The globular bodies dispersed through the stone, exhibited nothing magnetic.

Analysis.

Under all circumstances, I am induced to state the proportions of constituent parts thus:

Constituent parts.

Silica

Silica - - - -	50
Magnefia - - - -	15
Oxide of iron - - -	34
Oxide of nickel - - -	$2\frac{1}{2}$
	$101\frac{1}{2}$

Remarks.

The excefs of weight, inftead of the ufual lofs, is owing to the difference of oxidizement of the iron, in the ftone and in the refult of the analyfis; which will be found to be the cafe in all analyfes of thefe fubftances; indeed it is always neceffary to reduce the oxide to the red ftate, as being the only one to be depended upon. To avoid future repetition, I fhall alfo obferve, firft, that by preliminary experiments, I could not detect any other fubftance than thofe mentioned. Secondly, that the earth obtained as alumina, appeared to me to be moftly, if not intirely, filiceous; becaufe, after it had been ignited, and again treated with potafh and muriatic acid, I found it was very nearly all precipitated by evaporation. Thirdly, I examined, and judged of, the filica collected from the oxide of iron, in the fame way. Fourthly, the weight of the magnefia is given, not immediately, as obtained by evaporation, but after a fubfequent folution in an acid, and precipitation by potafh. And, fifthly, the proportions are taken from the mean of two analyfes.

The earthy matrix or cement.

Nothing remains to be examined, of the ftone from Benares, except the earthy matter, forming a cement or matrix for the fubftances already examined. 100 grains of this matter were, by mechanical means, feparated as perfectly as poffible, from the pyrites, iron, and globular bodies, and analyfed as above. The mean refult of two analyfes gave,

Its component parts.

Silica - - - -	48
Magnefia - - - -	18
Oxide of iron - - -	34
Oxide of nickel - - -	$2\frac{1}{2}$
	$102\frac{1}{2}$

Stone from Sienna.

EXAMINATION OF THE STONE FROM SIENNA.

The external coating of this ftone appeared to have the fame characters as that of the ftone from Benares.

The

The pyrites, although certainly prefent, were not cryftallized in fuch groups as in the preceding ftone; nor could they be feparated by mechanical means.

Iron and nickel as in the former inftance from the attractable metal.

The attractable metal was eafily feparated by the magnet; but $8\frac{1}{2}$ grains only were collected. I treated them with nitric acid and ammonia, as in a preceding cafe. Nearly one grain of earthy matter was infoluble; the weight was therefore reduced to rather lefs than 8 grains. The oxide of iron, precipitated by ammonia, weighed 8 grains; and the faline liquor gave abundant indications of nickel. As 8 grains of this oxide of iron contain nearly 6 of metal, the quantity of nickel, in the bare 8 grains, may be eftimated between 1 and 2 grains. Some globular bodies were extracted, but too few to analyze.

Analyfis of the ftone freed from magnetic metal and globular parts.

Since the pyrites could not be feparated, I collected 150 grains of the ftone, freed from iron by the magnet, and as exempt as poffible from globular bodies. Thefe 150 grains, I firft digefted with muriatic acid, that the pyrites might be decompofed, and every thing taken up which could be diffolved by that menftruum. A very decided difengagement of fulphureted hidrogen gas was occafioned. When the acid could produce no further action, I collected the undiffolved matter on a filtre, and boiled it with the moft concentrate nitric acid, in hopes of being able to convert the fulphur, previoufly liberated, into fulphuric acid; but my endeavours were fruitlefs; for, upon the addition of nitrate of barytes to the nitric folution, rendered previoufly tranfparent, a very infignificant quantity of fulphate of barytes was obtained. The furplus of barytic nitrate was removed by fulphate of potafh. I next completely edulcorated the mafs which remained infoluble, after the action of the muriatic and nitric acids; and, adding the water of edulcoration to the muriatic and nitric liquors, evaporated the whole for filica. I then fubmitted the mafs, undiffolved by the acids and the water, to the treatment with potafh, muriatic acid, and evaporation, which was, in the firft inftance, applied to the ftone from Benares. The firft precipitation was, as in that analyfis, alfo effected with carbonate of potafh; but, inftead of endeavouring immediately to extract alumina, I ignited the precipitate, that the alumina or filica remaining might be rendered infoluble. After the ignition, I feparated the oxide of iron with very concentrate muriatic acid; and the earths, which were left perfectly white,

I heated

I heated with potafh, until they were again capable of being taken up by the fame acid. The folution fo made, was flowly evaporated; and, as very nearly every thing was depofited during the evaporation, I conclude all was filica. The proportions refulting from this fingle analyfis, without the weight of fulphur contained in the pyrites irregularly diffeminated through the whole, were,

Its component parts.

Silica	70
Magnefia	34
Oxide of iron	52
Oxide of nickel	3
	159.

The ftone from Yorkfhire.

EXAMINATION OF THE STONE FROM YORKSHIRE.

The mechanical feparation of the fubftances in this ftone being as difficult as in the preceding cafe, I was neceffarily fatisfied with fubmitting it to the fame treatment. I collected, however, 34 grains of malleable particles; which, by the procefs already more than once mentioned, left 4 grains of earthy matter; and, by yielding 37½ of oxide of iron, indicated about 4 grains of nickel.

Component parts.

150 grains of the earthy part of the ftone were, by analyfis, refolved into,

Silica	75
Magnefia	37
Oxide of iron	48
Oxide of nickel	2
	162.

The ftone from Bohemia.

EXAMINATION OF THE STONE FROM BOHEMIA.

The probability of never being able to obtain another fpecimen of the very remarkable fragment of this fubftance, did not allow me to trefpafs more on the liberality of Mr. Greville, than to detach a fmall portion. I found it of fimilar compofition to that of the three preceding ftones; and the Count de Bournon has already fhewn the proportionate quantity of the attractable metal to be very confiderable. 16½ grains, left 2½ of

of extraneous earthy matter; and yielded, by the treatment with nitric acid and ammonia, $17\frac{1}{2}$ grains of oxide of iron. This would seem to iuduce an estimation of $1\frac{1}{2}$ of nickel in 14 grains, or about 9 per cent.

55 grains of the earthy part of the stone, by the analytical treatment of the two former, afforded, Component parts.

Silica - - - -	25
Magnesia - - - -	$9\frac{1}{2}$
Oxide of iron - - -	$23\frac{1}{2}$
Oxide of nickel - - -	$1\frac{1}{2}$
	$59\frac{1}{2}$

The unusual increase of weight in the result of the three last analyses, notwithstanding the intire loss of the sulphur in the pyrites, is obviously owing to the metallic state of the iron combined with the sulphur, as was shewn in a former instance.

Remarks on these analyses: compared with those formerly made by others.

I have now concluded the chemical examination of these four extraordinary substances. It unfortunately differs from the analysis made by the French Academicians, of the stone presented to them by the Abbé Bachelay. as well as from that made by Professor Barthold, of the stone of Ensisheim. It is at variance with that of the Academicians, inasmuch as they found neither magnesia nor nickel. It differs from that of Mr. Barthold, as he did not find nickel, but discovered some lime, with 17 per cent. of alumina. With regard to these differences, I have to submit to the chemical world, whether magnesia might not have eluded the action of an acid, when the aggregation of the integrant parts of the stone was not destroyed by treatment with potash. As to the existence of alumina, I do not absolutely deny it; yet I must observe, that the whole of the earth which seemed to have any resemblance, however small, to alumina, was at most 3 per cent. and there seems good reason to consider it as silica. Respecting the existence of lime in the stone of Ensisheim, I must appeal to Professor Barthold, whether, supposing lime a constituent part, sulphate of lime should not have been formed, as well as sulphate of magnesia, when sulphuric acid was generated by igniting the earths and pyrites. And, as to the proportion of alumina, in the same stone, I would ask, at least, whether it would have been so considerable, if the solutions formed by acids, after the treatment with potash, had been evaporated to

to the requiſite dryneſs: not to obſerve that no mention is made of any examination of the properties of the earth called alumina. In the proportion of magneſia, I have the ſatisfaction to find my analyſis correſpond very nearly with that of Profeſſor Barthold; and, if what he conſidered alumina were ſuppoſed ſilica, the ſtone preſented to the French Academy, the ſtone of Enſiſheim, and the four I have examined, would agree very nearly in ſiliceous proportions. With reſpect to the nickel, I am confident it would have been found in all, had the metallic particles been ſeparately examined. But, whatever be theſe variations, the mineralogical deſcription of the French Academicians, of Mr. Barthold, and of the Count de Bournon, all exhibit a ſtriking conformity of character, common to each of theſe ſtones; and I doubt not but the ſimilarity of component parts, eſpecially of the malleable alloy, together with the near approach of the conſtituent proportions of the earths contained in each of the four ſtones, the immediate ſubject of this Paper, will eſtabliſh very ſtrong evidence in favour of the aſſertion, that they have fallen on our globe. They have been found at places very remote from each other, and at periods alſo ſufficiently diſtant. The mineralogiſts who have examined them, agree that they have no reſemblance to mineral ſubſtances, properly ſo called; nor have they been deſcribed by mineralogical authors. I would further urge the authenticity of accounts of fallen ſtones, and the ſimilarity of circumſtances attendant on ſuch phenomena; but, to the impartial it would be ſuperfluous, and, to thoſe who diſbelieve whatever they cannot explain, it would be fruitleſs. Attempts to reconcile occurrences of this nature with known principles of philoſophy, it is true, are already abundant; but (as the Earl of Briſtol has well expreſſed) they leave us a choice of difficulties equally perplexing. It is however remarkable, that Dr. Chladni, who ſeems to have indulged in theſe ſpeculations with moſt ſucceſs, ſhould have connected the deſcent of fallen ſtones with meteors; and that, in the narrative of Mr. Williams, the deſcent of the ſtones near Benares, ſhould have been immediately accompanied with a meteor.

The ſtriking conformity of character in theſe ſtones, and their want of reſemblance to all other minerals, join with the hiſtorical teſtimony to prove their origin.

Luminous appearance attending the fall of ſeveral.

No luminous appearance having been perceived during the day on which the ſtone fell in Yorkſhire, it muſt be admitted, rather militates againſt the idea, that theſe ſtones are the ſubſtances which produce or convey the light of a meteor, or that

a meteor muſt neceſſarily accompany them *. Yet the ſtone from Sienna fell amidſt what was imagined lightning, but what might in reality have been a meteor. Stones were alſo found after the meteor ſeen in Gaſcony, in July, 1790. And Mr. Falconet, in the memoir I have already quoted, relates, that the ſtone which was adored as the mother of the gods, was a Bœtilia; and that it fell at the feet of the poet Pindar, enveloped in a ball of fire. He alſo obſerves, that all the Bœtilia had the ſame origin.

Electricity renders theſe ſtones luminous and black.

I ought not perhaps to ſuppreſs, that in endeavouring to form an artificial black coating on the interior ſurface of one of the ſtones from Benares, by ſending over it the electrical charge of about 37 ſquare feet of glaſs, it was obſerved to become luminous, in the dark, for nearly a quarter of an hour; and that the tract of the electrical fluid was rendered black. I by no means wiſh to lay any ſtreſs upon this circumſtance; for I am well aware, that many ſubſtances become luminous by electricity.

But, ſhould it ever be diſcovered that fallen ſtones are actually the bodies of meteors, it would not appear ſo problematical, that ſuch maſſes as theſe ſtones are ſometimes repreſented, do not penetrate further into the earth: for meteors move more in a horizontal than in a perpendicular direction; and we are as abſolutely unacquainted with the force which impels the meteor, as with the origin of the fallen ſtone.

Meteor which lately croſſed the county of Suffolk.

Before I cloſe this ſubject, I may be particularly expected to notice the meteor which, a few months ago, traverſed the county of Suffolk. It was ſaid, that part of it fell near Saint Edmundſbury, and even that it ſet fire to a cottage in that vicinity. It appeared, from inquiries made on the ſpot, that ſomething, ſeemingly from the meteor, was, with a degree of reaſon, believed to have fallen in the adjacent meadows; but the time of the combuſtion of the houſe did not correſpond with the moment of the meteor's tranſition.

Prodigious meteor in America.

A phenomenon much more worthy of attention, has ſince been deſcribed in the Philoſophical Magazine. On the night of the 5th of April, 1800, a body wholly luminous, was ſeen, in America, to move with prodigious velocity. Its apparent

* In the account of the ſtone which fell in Portugal, no mention is made, either of a meteor or lightning.

ſize was that of a large houſe, 70 feet long; and its elevation above the ſurface of the earth, about 200 yards. The light produced effects little ſhort of ſun-beams; and a conſiderable degree of heat was felt by thoſe who ſaw it, but no electric ſenſation. Immediately after it diſappeared in the north-weſt, a violent ruſhing noiſe was heard, as if the phenomenon were bearing down the foreſt before it; and, in a few ſeconds after, there was a tremendous craſh, cauſing a very ſenſible earthquake. Search being afterwards made in the place where the burning body fell, every vegetable was found burnt, or greatly ſcorched, and a conſiderable portion of the ſurface of the earth broken up. We have to lament, that the authors of this account did not ſearch deeper than the ſurface of the ground. Such an immenſe body, though moving in a horizontal direction, could not but be buried to a conſiderable depth. Should it have been more than the ſemblance of a body of a peculiar nature, the lapſe of ages may perhaps effect what has now been neglected; and its magnitude and ſolitary ſituation become the aſtoniſhment of future philoſophers.

Concerning the immenſe maſs of native iron of S. America.

This leads me to ſpeak of the ſolitary maſs of what has been called native iron, which was diſcovered in South America, and has been deſcribed by Don Rubin de Celis. Its weight was about 15 tons. The ſame author mentions another inſulated maſs of the ſame nature. The whole account is exceedingly intereſting; but, being already publiſhed in the Philoſophical Tranſactions for the year 1788, it needs not be here repeated.

Mr. Prouſt has ſhewn the maſs particularly deſcribed, not to be wholly iron, but a mixture of nickel and iron. The Truſtees of the Britiſh Muſeum, who are in poſſeſſion of ſome fragments of this maſs, ſent to the Royal Society by Don Rubin de Celis, have done me the honour to permit me to examine them; and I have great ſatisfaction in agreeing with a chemiſt ſo juſtly celebrated as Mr. Prouſt.

and that deſcribed by Pallas, and conſidered by the Tartars as fallen from heaven.

The connection which naturally exiſts between one maſs of native iron and another, immediately turns our attention to the native iron in Siberia, deſcribed by Pallas; and this, we are told, the Tartars conſidered as a ſacred relic, which had dropped from heaven. The nickel found in the one maſs, and the traditional hiſtory of the other, not to compare the globular bodies of the ſtone from Benares with the globular concavities

concavities and the earthy matter of the Siberian iron, tend to the formation of a chain between fallen ftones and all kinds of native iron. How far any real affinity exifts between thefe feveral fubftances, very obliging friends have afforded me an opportunity to form fome judgment. I am indebted to Mr. Greville and Mr. Hatchett for portions of almoft every known native iron: and the Count de Bournon has done me the favour particularly to defcribe them as follows.

(To be concluded in our next.)

V.

On the Colours obtained from Metallic Oxides, and fixed by Means of Fufion on different Vitreous Bodies. By ALEX. BRONGNIART, *Director of the National Manufactory of Porcelain at Sévres, Engineer of Mines* *, *&c.*

THE art of employing metallic oxides to colour the different vitreous matters has been long known. It is well known that the ancients made coloured glafs and enamels, and that this art was much practifed by the Egyptians, who were the firft that imitated precious ftones by thefe means.

The colouring of glaffes by oxides is very ancient.

In modern times the practice of this art has been brought to a high degree of perfection, but its theory has been neglected, It is almoft the only chemical art to which the new principles of this fcience have not been applied.

The theory neglected,

In the numerous works which treat of the method of ufing and preparing metallic vitrifiable colours, the authors either give no theory, and confequently no general principles, or the explanations are founded only on the abfurd hypothefes which formerly compofed much of the theory of chemiftry.

or erroneous.

One of the beft publications, as it is the work of an enlightened practician, is *The Treatife on Painting in Enamel, of Montamy*. The archives of the national manufactory at Sévres, likewife contain fimple and good proceffes for the fabrication of colours; Bailly, Fontelliau, and Montigny are the authors; but they are fimple defcriptions, without any obfervations that lead to general principles.

Montamy's treatife commended.

* Journal des Mines, No. 67.

Kunckel's

Kunckel's work and the Encyclopædias are of little value.

Kunckel's work, the manuſcripts of Hellot, in the poſſeſſion of the manufactory at Sévres, and the two Encyclopædias, preſent only an undigeſted collection, and a multitude of proceſſes collected together from all quarters, without choice or reaſon. With a ſlight knowledge of the art it is eaſier to invent a new proceſs of fabrication, than to diſcover which among this multitude of receipts we ought to prefer.

Science is conſiderably advanced when the facts compoſe a body of doctrine.

It has been remarked, that one of the moſt certain criterions of the progreſs made to the perfection of a ſcience, is the poſſibility it leaves of a re-union of the facts that compoſe it, in a body of doctrine from which general principles may be deduced. At the preſent period only does it deſerve the name of a ſcience, and it is to the expoſition of theſe principles, that the ſtriking, though inaccurate name of the philoſophy of ſcience has been given.

The arts would be greatly advanced by ſimilar treatment.

The arts, which oftener compoſe a branch of ſcience, than the ſimple application of one of its parts, preſent facts equally capable of being united in a body of doctrine; they will attain this valuable degree of perfection if practiſed by men accuſtomed to trace the connections, and deduce the conſequences of the events that paſs under their obſervation.

Philoſophers might acquire them.

Philoſophers, whoſe more elevated ſpeculations ſeclude them from the practice of theſe arts, would then perceive their principles with greater eaſe; they could more directly apply their reſearches to the progreſs of the art, which being then directed by reaſon, would become more certain, direct and rapid in its advancement.

The preſent memoir aims to make this diſpoſition as to the art of vitrifiable colours.

Being convinced that the art of preparing and uſing vitrifiable colours is ſuſceptible of receiving the application of the means of perfection; and that the facts of which it is compoſed are ſufficiently numerous and accurate to be preſented in a general outline, I have concluded that the preciſe knowledge of theſe facts, and of the principles which unite them, which naturally lead to an explanation of a number of the reſults, might be intereſting to chemiſts, who, being occupied by more general and important reſearches, cannot attend to all the details of a complicated art.

I wiſhed likewiſe to give an accurate account of the principles of this art to chemiſts, that they might determine with certainty on the new operations to which the proceſſes ſubmitted to their judgment might lead.

Laſtly,

Laſtly, I thought it would be advantageous to the progreſs of the art, and that it was incumbent on the national manufactory at Sèvres to diſcloſe the pretended ſecret of the compoſition of colours for porcelain which do not change in the fire. Theſe colours were preſented to the Inſtitute in the year 6, by a manufacturer of porcelain, juſtly admired for the beauty of the works produced at his manufactory. I ſhould not have publiſhed this ſecret had it been given to me in confidence, but as it is known, I hope I ſhall no longer be ſuſpected of the leaſt breach of integrity. Exhibition of the ſecret of the colours at Sévres.

From what has been ſaid, it will be ſeen that my object is not to give the exact compoſition of all the vitrifiable colours at full length. Such a work could not be the ſubject of a ſimple memoir.

It is known that metallic oxides are the baſes of all vitrifiable colours; but ſome of the metallic oxides are not proper for this uſe; and as they are not vitrifiable without ſome admixture, they can ſeldom be employed alone. Vitrifiable colours are made by met. oxides.

The very volatile oxides, and thoſe that do not adhere ſtrongly to the oxigen they contain in abundance, either cannot be employed in any manner, as is the caſe with the oxides of mercury and arſenic, or elſe they can be employed only as agents. The colour they afford cannot be depended on, becauſe they loſe it, in proportion as they loſe their oxigen by the ſlighteſt heat. Such are the puce and red coloured oxides of lead, the yellow oxide of gold, &c. Not thoſe which are volatile, or eaſily change their oxigenation.

Thoſe oxides, in which the proportion of oxigen is capable of being varied too eaſily, are ſeldom employed. The black oxide of iron is never employed for that colour. The green oxide of copper is in many circumſtances very uncertain in its effect.

I have remarked that the oxides do not melt alone; though when placed in thin layers on vitrifiable ſubſtances they can be made to adhere by means of a violent fire; but, with the exception of the oxides of lead and biſmuth, they afford only dull colours. The very violent heat which is frequently required to fix them, changes or totally deſtroys their colours. Some flux is therefore added to all the metallic oxides. Oxides cannot be uſed alone;

This flux is either glaſs of lead and ſilex, or glaſs of borax, or a mixture of both. but require a flux. Lead and ſilex, or lead, ſilex, and borax.

Its

The flux fixes the colour at a moderate heat; less than would destroy the colour.

Its general effect is to give brilliancy to the colours after their fusion, to fix them upon the piece that is painted by softening its surface, to envelope the metallic oxides, to preserve their colour by defending them from the contact of the air, and particularly to facilitate the fusion of the colour at a slightly elevated temperature, not capable of destroying it.

The observation which proves this latter use of solvents is taken from delicate colours, such as the carmines produced from gold; these colours require a much greater proportion of flux than the others.

Very transient colours are mixed with the flux without previous fusion.

Sometimes the oxides are directly employed, and simply mixed with their solvents, without having been previously melted with it; such are the colours which the violence or repetition of heat would alter. It is easy to conceive that a stronger and more lasting heat is required to fuse a crucible of coloured glass than a layer of colour, which is not the tenth of a millimetre in thickness.

I shall return to this subject when treating of the red colours obtained from gold.

Others are fused with it and then ground.

In many cases the oxides are previously fused with their solvents, and afterwards ground. When I speak of colours in particular, I shall mention those which are subjected to this fusion.

These general principles are too simple to require further explanation.

This treatise is confined to vitrifiable colours on glazes or glass.

I shall only speak of the application of metallic colours to vitreous bodies or surfaces.

These grounds are three.

These bodies may be divided into three very distinct classes, from the nature of the substances that compose them, the effects produced on them by the colours, and the changes they undergo. These are,

1. Enamel.

1st. Enamel; soft porcelain, and all the glazes, enamels, or glasses, which contain lead in any considerable quantity.

2. Hard porcelain.

2d. Hard porcelain, or such as is glazed with feldspar.

3. Common glass.

3d. Glass in which there is no lead, such as the common window glass.

I shall successively examine the principles of composition of these colours, and the general phenomena they present on these three grounds or supporters.

Colours

Colours for enamel painting have been longeſt known: the receipts in the works I have mentioned at the beginning of this eſſay all relate to theſe colours. Enamel colours have been long known.

It is known that enamel is a glaſs rendered opake by oxide of tin, and very fuſible by the oxide of lead. It is this laſt which in particular gives it properties very different from thoſe of the other excipients of metallic colours. Hence all the glaſſes and glazes that contain lead, have the properties of enamel, and what we may aſſert of the one, will apply to the other with very little difference. Enamel is glaſs rendered fuſible by ox. lead, and opake by ox. tin.

Such are the white and tranſparent glazes of Dutch or Delft ware, and the glaze of the porcelain called ſoft *(tendre.)* It is the glaze of Delft ware, &c.

This porcelain, the firſt made in France, particularly at Sévres, and indeed for a long time almoſt excluſively at this manufactory, has for its baſe a vitreous frit nearly opake, capable of being acted upon by marl, and its glaze is a very tranſparent glaſs containing much lead. Soft porcelain of Sévres.

The colours made uſe of are the ſame as thoſe for enamelling, conſequently the changes theſe colours undergo in enamel, muſt take place in this ſpecies of porcelain, becauſe, as we ſhall ſoon ſee, the cauſes of the change are the ſame in both. Colours the ſame as for enamelling.

The colours for enamel and tender porcelain require leſs flux than the others, becauſe the glaſs on which they are placed, ſoftens ſufficiently to be penetrated by them. Theſe require leſs flux becauſe the glaze is very fuſible.

This ſolvent may be either the mixture of glaſs of lead, and pure ſilex called *rocaille,* or this ſame glaſs mixed with that of borax. The flux before mentioned.

Montamy ſays that glaſs of lead ought not to be uſed in the flux for enamel; he employs borax alone. He then dilutes or makes up his colours in a volatile oil.

On the contrary, the painters of the manufactory at Sévres, uſe only colours without borax, becauſe they dilute them with gum, and borax does not dilute well this way. I am convinced that both methods are equally good, and that Montamy is not juſtified in excluding the fluxes of lead, as they are employed without inconvenience every day, and even render the management of colours more eaſy. Glaſs of lead is a good material.

I have remarked, that in the baking of theſe colours the glaze is ſoftened ſo much as to be eaſily penetrated by them; this is one great cauſe of the change they undergo. They be- The ſoft glaze very much dilutes the colours.

 come

come diluted by the mixture with the glaze, and the firſt fire changes a painting apparently finiſhed, into a very ſlight ſketch.

And the oxide of lead acts chemically on them.

The oxide of lead contained in the glaze is a more powerful cauſe of the great changes theſe colours undergo. Its deſtructive action is principally exerciſed on the reds of iron, and is very remarkable. I ſhall relate ſome experiments that particularly prove this.

Theſe colours themſelves are ſcarcely changeable, except from the ground they are placed upon.

It has already been ſhewn that the two principal cauſes of the change which colours on enamel and tender porcelain undergo, do not relate to the compoſition of theſe colours, but intirely to the nature of the glaſs upon which they are placed. The aſſertion that the colours of porcelain are ſubject to conſiderable change, relates to the colours of ſoft porcelain, *(porcelain tendre)* a ſpecies of ware now almoſt totally abandoned.

The paintings before mentioned require much touching up.

It follows from what I have ſaid, that the paintings of porcelain require to be ſeveral times re-touched and burned, in order to poſſeſs the neceſſary ſtrength. Though theſe paintings have always a certain ſoftneſs, they are conſtantly more brilliant, and never ſubject to the inconvenience of ſcaling off.

Hard porcelain; with glaze of feld ſpar,

The hard porcelain, according to the diviſion I have laid down, is the ſecond ſpecies of ground or excipient for the metallic colours. It is known that the baſe of this porcelain is a very white argil, called *kaolin,* mixed with a ſiliceous and calcareous ſolvent, and the glaze of which is nothing but feld ſpar fuſed without an atom of lead.

or of Saxony, receives two kinds of colour; viz. on the glaze or painting colours,

This porcelain, which is that of Saxony, is of much later date at Sévres than the ſoft or tender. The colours employed are of two kinds, the firſt uſed for repreſenting different objects, are baked with a very inferior fire to that required for the baking of the porcelain itſelf. They are very numerous and varied.

or beneath it; or ground colours.

The others, which require to be fuſed at as great a heat as that for baking the porcelain, are laid on the general ſurface. They are much leſs numerous.

The colours for painting are nearly the ſame as for the ſoft porcelain; but contain more flux.

The colours for painting are made up very nearly of the ſame materials as thoſe for tender porcelain; they only contain more flux. This flux is compoſed of the glaſs of lead, (called *rocaille*) and of borax. I have not yet met with any work that treats of the compoſition, uſe and effects of theſe colours. In fact, it has no where been aſſerted in print, that all

all theſe colours except one are unchangeable in the fire; whereas it has been often aſſerted in books, that paintings in enamel are ſubject to conſiderable change.

The flux is glaſs of lead and borax.

When the porcelain is put in the fire to bake the colours, the feld ſpar glaze dilates and opens its pores, but does not become ſoft. As the colours do not penetrate it, they are not ſubject to the changes they undergo on tender porcelain. It muſt however be obſerved, that they loſe a little of their intenſity, by acquiring the tranſparence given them by the fuſion.

The hard glaze does not run nor dilute the colours.

When works of little importance are made they need not be re-touched; but this is neceſſary when a painting is to be highly finiſhed. This re-touching is not diſtinguiſhable in the paintings on porcelain from that of any other ſpecies of painting.

ſo that only fine work needs re-touching.

One of the great inconveniences of theſe colours, is the facility with which they ſcale or fly off when the fire is often applied.

The colours are apt to ſcale.

This has been particularly remarked at Sévres, on account of the ſolidity and infuſibility with which porcelain is there manufactured. But theſe qualities cauſe it to reſiſt the alternations of heat and cold for a longer time, and gives its ground a more brilliant white colour. On the other hand, the porcelains of Paris being more vitreous, tranſparent, and of a blueiſh caſt, generally crack if boiling water is frequently poured into them.

Hard porcelain is more beautiful, and ſtands changes of heat and cold better than the ſoft.

In order to remedy this evil without altering the quality of the body, I ſoftened the glaze a little, by introducing more ſiliceous or calcareous flux according to the nature of the feld ſpar. This method ſucceeded, and for this twelvemonth paſt the colours have paſſed two and three times through the fire without cracking, provided there be not too much flux, and they be not laid on too thick.

The ſcaling is prevented by ſoftening the glaze.

It has been remarked, that when ſoda and potaſh were introduced the colours ſcaled; ſo that they cannot be uſed as fluxes. Theſe alkalies being volatilized abandon the colours, which cannot adhere to the glaze by themſelves.

Alkalies are too volatile to be uſed in this art.

I have obſerved that other colours are likewiſe prepared, which being laid upon the general ſurface, are fuſed by the ſame fire as bakes the porcelain. Theſe colours are but few; becauſe there are few metallic oxides that can ſupport ſuch a

Colours to be laid on and baked by the firſt fire are few.

fire

fire without being volatilized or difcoloured. Their folvent is the feld fpar. As they incorporate with the glaze they never crack, and are more brilliant.

The third receptacle of metallic vitrifiable colours is glafs without lead.

Art of painting on glafs, is not loft.

The application of thefe colours conftitutes the art of painting on glafs; an art much practifed in former ages, but which is fuppofed to be loft, becaufe out of fafhion. It however too immediately depends on the art of painting on enamel and porcelain to be intirely loft. Defcriptions of the proceffes may alfo be found in many different works.

Books in which it is treated. Original.

A book intitled, *L'Origine de l'Art de la Peinture fur Verre*, publifhed at Paris in the year 1693, and *Le Traité de l'Art de la Verrerie*, by *Neri* and *Kunckel*, feem to be the firft works containing complete defcriptions of this art. Thofe publifhed

Leviel's work is only a compilation.

fince, even the great work of *Leviel*, which conftitutes part of *Les Arts et Metiers* of the academy, and of the *Encyclopédie Methodique*, are only compilations from the two former works.

The others afford very loofe inftruction.

It is fomewhat remarkable, that if we follow the proceffes exactly as they are defcribed in thefe works, as I have done with fome of them, the colours of which they pretend to give the receipt would never be fabricated. They only ferve to fhew an able practician the method, and leave it to him to correct or make additions. This was found to be the cafe by Citizen Meraud, who was engaged to prepare them for the manufactory of Sévres. He was obliged to make the colours for painting on glafs rather from his own experience, than from the inftructions in the works I have juft mentioned.

The colours for glafs are nearly the fame as for enamel; but fome of them are dull for want of the opake ground.

The limits of a memoir will not permit me to enter into hiftorical details on the art of painting on glafs; its hiftory is given at length in Leviel's work; the materials and fluxes which enter into the compofition of the colours for painting on glafs, are in general the fame as thofe applied to porcelain. They vary only in their proportions; but a great number of the colours ufed for enamel and porcelain cannot be applied to glafs; many of them when feen by tranfmitted light intirely change their afpect, and exhibit an obfcure tint which can be of no ufe when deprived of the white ground which throws them out. We fhall point out thefe when we treat of the colours in particular. Thofe colours which can be ufed on

this

this body, ſometimes change in the baking, and acquire a great tranſparence. They are generally beautiful only when placed between the eye and the light, and then they anſwer the purpoſe intended in painting on glaſs.

There is more difficulty in baking plates of coloured glaſs than is commonly thought. The bending of the piece, and alteration of the colours, are to be avoided. All the treatiſes we have conſulted recommend the uſe of gypſum. This method ſometimes ſucceeded, but generally the glaſs became white, and cracked in all directions. It appears that the glaſſes that are too alkaline, and which are far the moſt common in clear white glaſſes, are attacked by the hot ſulphuric acid of the ſulphate of lime. We were able with eaſe to bake much larger glaſſes than any before painted, by placing them on very ſmooth plates of earth or unglazed porcelain.

Difficulties of baking glaſs plates.

On the uſual ſupport of gypſum, the alkali of the glaſs is attacked, and the piece broken.

Porcelain biſcuit is a much better ſupport.

Concerning the ſeveral particular Colours.

After having collected the general phenomena which each claſs of vitrifiable colours offers with regard to the bodies on which they are placed, I muſt ſhew the particular and moſt intereſting phenomena which every principal ſpecies of colours employed on tender porcelain, on glaſs, and in the fire that bakes the porcelain, preſent.

Particular colours.

Concerning the Reds, Purples, and Violets obtained from Gold.

The carmine red is obtained from the purple precipitate of Caſſius. It is mixed with about ſix parts of its flux; and this mixture is directly employed without being firſt fuſed. It is then of a dirty violet, but acquires the beautiful carmine by baking. It is however very delicate; a little too much heat or carbonated vapours eaſily ſpoil it; yet it is more beautiful when baked with charcoal than with wood.

Carmine red from gold, is very periſhable in the fire,

This colour and the purple which differs little from it, as well as all the ſhades which are obtained from their mixture with other colours, really change in all porcelains and in the hands of all operators. But this is the only one that changes on hard porcelain. It may be replaced by a ſubſtitution of roſe colour from iron, which does not change; ſo that by excluding from the pallet the carmine made from gold, and ſubſtituting the roſe-

as well as its ſhades.

It is the only colour that changes on hard porcelain;

and may be changed for oxide of iron in this ufe, though this oxide fails *on enamel.*

rofe-coloured oxide of iron here fpoken of, we have a pallet compofed of colours none of which are fubject to any remarkable change. This rofe-coloured oxide of iron has been long known, but was not employed *on enamel,* becaufe it is then fubject to confiderable change. Or perhaps when the painters on enamel became painters on porcelain, they continued to work according to their ancient method.

We might fuppofe that by previoufly reducing the colour named carmine, already mixed with its folvent, into a vitreous matter, the laft tint would be obtained. But as I have already proved, the fire which muft be ufed to melt this vitreous mafs deftroys the red colour. Befides, it is found that, to obtain this colour in perfection, it is neceffary to pafs it through the fire as little as poffible.

Fabrication of the gold carmine.

The carmine of tender porcelain is made with fulminating gold gently decompofed, and muriate of filver; there is no tin in it, which proves that it is not neceffary for the fabrication of a purple colour, that the oxide of this laft metal and that of gold fhould be combined.

Violet from gold ftands better on foft porcelain.

Violet is likewife obtained from the purple oxide of gold. This colour proceeds from having a greater quantity of lead in the flux, and it is nearly of the fame tint whether crude or baked.

But they all fail on hard.

Thefe three colours totally difappear in the ftrong fire proper to bake porcelain.

The carmine and purple are not good upon glafs; but the violet is beautiful.

Carmine and purple afforded us upon glafs, only tints of a dirty violet. The violet, on the contrary, has a very beautiful effect; but it is fubject to change to a blue. I cannot yet afcertain the caufe of fo fingular a change, as I obferved it for the firft time only a few days ago.

(To be concluded.)

VI. *Examination*

VI.

Examination of the supposed igneous Origin of the Rocks of the Trapp-formation. By Mr. Robert Jameson. *From the Author.*

Sheriff Brae, Leith, Sept. 8, 1802.

That the Rocks of the Trapp-formation have been deposited from a State of Solution and Suspension in Water, is a Truth, which has been demonstrated by the Wernerian Geognosie.

Aqueous origin of the rocks of trapp-formation.

IT is not at present my intention to state the proofs of this opinion: the subject of the following observations is the examination of Mr. Playfair's account of these rocks.

Prof. Playfair's statement: that the columnar form of basalt is the effect of fire, because it is seen in lavas.

At section 64 of his illustrations of the Huttonian theory, he observes, "The columnar structure sometimes found in that species of whinstone called basaltes, is a fact which has given rise to much discussion; and it must be confessed, that though one of the most striking and peculiar characters of this fossil, is not that which gives the clearest and directest information concerning its origin. One circumstance, however, very much in favour that basaltic rocks owe their origin to fire, is, that the columnar form is sometimes assumed by lava actually erupted from volcanos. Now, it is certainly of no small importance to have the synthetic argument on our side, and to know that basaltic columns can be produced by fire; though no doubt, to give absolute certainty to our conclusion, it would be necessary to shew, that there are in nature no other means but this by which these columns can be formed. This sort of evidence can hardly be looked for; but since the power of fusion to produce the phenomena in question is perfectly established, and since the production of the same phenomena in the humid way is a mere hypothesis, if there be the least reason to suspect the action of subterraneous heat * as one of the causes of mineralization,

* The phenomena of volcanos and hot springs are considered by Professor Playfair as proofs of a subterranean heat, unconnected with the decomposition of any mineral substance. The Wernerian geognosie, however, has shewn us, that volcanos and hot springs originate in the newest flotz trapp-formation, therefore we have sufficient

Reply. That the descriptions of lavas by Spallanzani and others, are incorrect as to the appearance and the matter itself; and that the aqueous origin of basalt is deducible from similarity of structure in other fossils so formed.

ralization, every maxim of sound philosophy requires that the basaltic structure, in all cases, should be ascribed to it." The accounts given by Spallanzani and other travellers of columnar lava, are most evidently incorrect; the appearances they have described are accidental rents, such as we observe in sandstone, limestone, and even in basalt. Many of the prismatic lavas described by Spallanzani, are either basalt, greenstone, or porphyry. That basalt columns have been formed by chrystallization from a state of solution in water, is rendered probable from the analogy of their structure with that of other fossils which have been certainly so formed. Basalt we know occurs in the same mass in tabular and columnar distinct concretions, corresponding in this particular with the lamellar and columnar distinct concretions of truly chrystalline fossils *.

P. The presence of pyrites urged as the proof of igneous production;

At section 66 we are informed, "That a mark of fusion, or at least of the operation of heat, which whinstone possesses in common with many other fossils, is its being penetrated with pyrites, a substance, as has been already remarked, that is of all others most exclusively the production of fire."

but these are very abundant among matters never subjected to fire.

Mr. Playfair probably did not know that the great beds of bituminated wood which are found covered with sand, clay, &c. in the alluvial hills, are very frequently much impregnated with pyrites; and that lately Mr. Hatchett had also demonstrated its formation in the humid way.

P. Insulated masses of indurated sandstone in whin, urged as a proof of igneous fusion.

He continues, "Another mark of fusion, more distinctive of whin, is, that both in veins and masses it sometimes includes pieces of sandstone, or of the other contiguous strata, completely insulated, and having the appearance of fragments of rock floating in a fluid sufficiently dense and ponderous to sustain their weight. Though these fragments have been too refractory to be reduced into fusion themselves, they have not remained intirely unchanged; but are, in general, extremely indurated, in comparison of the rock from which they appear

cient proof of the existence of quantities of inflammable matter, equal to the support of all the volcanoes that have, or can exist on our globe.

* When basalt, having a columnar structure, occurs in veins, we find that these columns are perpendicular to the walls of the vein. This phenomena I have frequently observed, and consider it as supporting in some degree the opinion mentioned above.

to

to have been detached." That the pieces of sandstone are not insulated, I have proved in a former paper. The occurrence of masses of sandstone in veins of basalt, grunstone, and wacken, is well known; but that there are masses more indurated than the other sandstone of this formation, must be denied, as we know that sandstone beds occur in this formation of all degrees of induration. It is denied that these are insulated or indurated.

Section 67. "Similar instances of extraordinary induration are observed in the parts of the strata in contact with whinstone; whether they form the sides of the veins, or the floors and roofs of the masses into which the whinstone is distributed. The strata, whether sandy or argillaceous, in such situations are usually extremely hard and consolidated; the former in particular lose their granulated texture, and are sometimes converted into perfect jasper. This interesting remark was first made by Dr. Hutton, and the truth of it has been verified by a great number of subsequent observations." P. Extraordinary induration of the strata in contact with whinstone;

The flinty sandstone which is characteristic for the newest flötz trapp-formation, is usually covered by clay, wacken, basalt, or grunstone, in the order here mentioned: sometimes the clay is wanting, when the sandstone is covered by wacken, or when the wacken has been carried away, or not deposited, by basalt or grunstone. We have thus, to use the Huttonian language, the sandstone equally indurated under a bed of clay as under one of basalt or grunstone: it is, therefore plain, that no argument can be drawn from the situation and nature of this sandstone in favour of the igneous system. When the walls of veins are indurated, we can easily trace it to the deposition of a portion of the basaltic solution in the pores of the softer strata. accounted for by the deposition of basaltic solution in the pores of softer strata.

Section 68. "To the same excellent geologist (Dr. Hutton) we are indebted for the knowledge of an analogous fact attendant on the passage of whinstone veins through coal strata. As the beds of stone where they are in contact with the veins of whin, seem to acquire additional induration, so those of coal, in like circumstances, are frequently found to have lost their fusibility, and to be reduced nearly to the state of coke or charcoal. The existence of coal of this kind has been already mentioned, and has been considered as a proof of the operation of subterraneous heat. In the instances here referred to, that is, where the charring of the coal is limited to those parts of the strata which are in contact with the whin, or in its immediate vicinity, P. The appearance of charring or coaking of coal in the vicinity of veins of whin, adduced as a proof of their former state of fusion.

vicinity, the heat is pointed out as refiding in the vein; and this is to be accounted for only on the suppofition of the melted whin, at a period fubfequent to the confolidation of the coal, having flowed through the openings of the ftrata. The heat has been powerful enough in many places to drive off the bituminous matter of the coal, and to force it into the colder and more diftant parts. Few facts in the hiftory of foffils are more remarkable than this, and none more directly affimilates the operations of the mineral regions with thofe that take place at the furface of the earth."

In reply. This effect is afcribed to the percolation of water.

An eminent Irifh geognoft is of opinion, that the appearances defcribed by Profeffor Playfair may have been produced by the percolation of water along the fides of the bafalt vein. This explanation will be fatisfactory to thofe who have witneffed the effects produced by the weather on coal. This altered coal, however, muft not be confounded with the glanz and columnar coal, becaufe thefe differ from it in oryctognoftical and geognoftical characters.

The argument contained in fection 69, is refuted by what Werner mentions in his admirable book on the formation of veins.

P. That the whinftone was introduced between the ftrata, is deducible from the fimilarity of the parts fo feparated.

Section 71. "Again, if it be true that the maffes of whinftone, thus interpofed among the ftrata, were introduced there after the formation of the latter, we might expect to find, at leaft in many inftances, that the beds on which the whinftone refts, and thofe by which it is covered, are exactly alike. If thefe beds were once contiguous, and have been only heaved up and feparated by the irruption of a fluid mafs of fubterranean lava, their identity fhould ftill be recognifed. Now this is precifely what is obferved; it is known to hold in a vaft number of inftances, and it is ftrikingly exemplified in the rock of Salifbury Crag near Edinburgh."

This argument affirmed to prove too much, becaufe it would apply to other middle ftrata.

If this argument was correct, it fhould follow, that the beds of foliated limeftone containing fhells, which are found between ftrata of fandftone, had been ejected from below. This fact cannot be evaded by faying, that the limeftone has been only foftened; it is plain if the foliated fracture (according to the Huttonian fyftem) is a character of fufion in one ftone, fo muft it in all others.

P. The nodules of agate in whinftone appear to have been fluid;

At fection 73 we have the following obfervations on the formation of agates: "Some of the fpecies of whinftone are the common matrices of agates and chalcedonies, which lie inclofed

in them in the form of round nodules. The original fluidity of theſe nodules is evinced by their figured and ſometimes chryſtallized ſtructure, and indeed is ſo generally admitted, that the only queſtion reſpecting them is, whether this fluidity was the effect of heat or ſolution. To anſwer this queſtion Dr. Hutton obſerves, that the formation of the concentric coats of which the agate is uſually compoſed, has evidently proceeded from the circumference toward the center, the exterior coats always impreſſing the interior, but never the reverſe. The ſame thing alſo follows from this other fact, that where there is any vacuity within the agate, it is uſually at the center, and there too are found the regular chryſtals, when any ſuch have been formed. It therefore appears certain, that the progreſs of conſolidation has been from the circumference inwards, and that the outward coats of the agate were the firſt to acquire ſolidity and hardneſs. Now it muſt be conſidered that theſe coats are highly conſolidated; that they are of very pure ſiliceous matter, and are utterly impervious to every ſubſtance which we know of, excepting light and heat. It is plain therefore, that whatever at any time, during the proceſs of conſolidation, was contained within the coats already formed, muſt have remained there as long as the agate was intire, without the leaſt poſſibility of eſcape. But nothing is found within the coats of the agate ſave its own ſubſtance; therefore no extraneous ſubſtance, that is to ſay no ſolvent, was ever included within them. The fluidity of the agate was therefore ſimple, and unaſſiſted by any menſtruum. In this argument nothing appears to me wanting that is neceſſary to the perfection of a phyſical, I had almoſt ſaid of a mathematical demonſtration. It ſeems, indeed, to be impoſſible that the igneous origin of foſſils could be recorded in plainer language, than the phenomenon which has been juſt deſcribed."

and to have conſolidated from without:

They are pure ſiliceous matter, impervious to any other ſubſtance;

whence this fluidity is inferred to have been ſimple, without any menſtruum.

It is well known to geognoſts, that during the depoſition of certain kinds of ſtrata, conſiderable quantities of air have been formed, and this in endeavouring to eſcape has given them a cellular ſtructure. The almond-ſtone (mandelſtein) in which agates are moſt frequently found, has acquired its cellular ſtructure in this manner. Theſe cells appear to have been ſucceſſively filled with a ſtony ſolution, out of which thin coats of jaſper, carnelian, chalcedony, &c. have been precipitated, according as the precipitate altered or was finer. We are na-

Reply. The cellular ſtructure of the ſtrata aſcribed to gas;

the cells are ſtated to have been ſucceſſively filled with a ſtony ſolution;

turally led to the idea of an increasing fineness in the solution, because we observe most distinctly that the outer coat is composed of the coarsest precipitate, and that as it approaches the middle, it is always finer and finer; so that at last, out of the finest solution, amethyst shoots, and, when there is room enough, chrystallizes. That the solution out of which the precipitation took place was truly chemical, is demonstrated not only by the character of the substances, but also by the disposition of the coats; for we can observe that they have followed the attraction of the walls of the cell, and are so accurately deposited upon it, that an inequality, or elevation upon the outer wall, continues to be observed through all the superincumbent coats to the center of the agate. That the solution had come from without, and by infiltration penetrated into the cells, is most distinctly to be seen in specimens of agate when they are properly cut. If the cutting has been judiciously done, we can always observe the opening by which the solution has entered. This elegant explanation is an outline of that delivered by the illustrious Werner in his geognostical lectures, and is a demonstration of the aqueous origin of agate.

whence the precipitation of nodules took place.

The opening of infiltration observable.

Flinty fossils not impervious to water.

That flinty fossils, as stated by Mr. Playfair, are impervious to water, has never been proved; on the contrary, it is well known to collectors of fossils, that if the cellular rock chrystal, which contains water, is kept in a cabinet for some years, the water gradually disappears.

Other obs.

Sections 249, 250, 1, 2, are occupied with an unsuccessful attempt to reconcile the appearances on the hill of Scheibenberg with the Huttonian theory. Without insisting upon the Wernerian doctrine of formations, which gives the death-blow to all partial explanations like those proposed by the Huttonian theory, I shall merely mention a few facts respecting the individual relation of these rocks, to shew the insufficiency of the explanation attempted by Professor Playfair.

Situations of wacken admitted by Prof. P.

1. Sandstone is sometimes observed covering wacken.

2. Gravel, covered by slaty clay and basalt, has been observed lying on wacken.

3. Veins of wacken sometimes traverse gneiss, porphyries, sandstone, &c.

adduced to prove that it was not hardened by fire.

Now, if wacken is clay hardened by the super-position of melted basalt, we must suppose the same effect to have been

4 produced

produced by the ſlaty clay, ſandſtone, or gravel. It is equally impoſſible to explain wacken veins by this hypotheſis of Profeſſor Playfair.

From ſection 255 to 259 is principally occupied in endeavouring to ſhew, that from the wedge-ſhape irregularities in thickneſs, and the frequent great inclination of beds of baſalt, they would not have been depoſited from a ſtate of ſolution in water.

Wedge-ſhape irregularities of baſalt,

To explain the appearances deſcribed in theſe ſections will be no difficulty to the Neptunian, they are what he daily meets with, and are illuſtrative of the depoſition of theſe ſtrata from water. Let us conceive a bed of baſalt depoſited upon an unequal ſurface, that the water breaks it (either before it has become ſolid, or afterwards) into ſhapes reſembling thoſe deſcribed by Profeſſor Playfair, afterwards that ſandſtone is depoſited upon this broken bed; and we have a complete picture and explanation of the wedge-ſhape, irregularities in the thickneſs, and every variety of inclination of the ſtrata of baſalt. This is illuſtrated by figure 1 and 2, Plate VIII.

accounted for by aqueous depoſition on an unequal ſurface; broken by water; and ſandſtone depoſited thereon.

A very ſtrong objection has been made to the igneous origin of baſalt and grunſtone, which has always appeared to me completely unanſwerable: it is this: If theſe rocks had been formerly in a ſtate of complete fuſion, they ſhould either appear as glaſs, or as a ſimple maſs, not containing any ſubſtance of a different nature from the maſs itſelf.

Objection to the igneous formation of baſalt, that it is not ſimple, but contains other bodies.

Mr. Playfair, at the concluſion of his obſervations on the trapp-formation rocks, remarks, "Notwithſtanding all this accumulated and unanſwerable evidence for the igneous formation of baſaltes, *(the evidence I have examined in the preceding pages)* a great objection would ſtill remain to our theory, were it not for the very accurate and concluſive experiments concerning the fuſion of this foſſil, made by Sir James Hall. A ſtrong prejudice againſt the production of any thing like a real ſtone by means of fuſion, had ariſen even among thoſe mineralogiſts who were every day witneſſes of the ſtony appearance aſſumed by volcanic lava. They ſtill maintained, on the authority of their own imperfect experiments, that nothing but glaſs can ever be obtained by the melting of earths and ſtones, in whatever way they are combined.

P. Sir James Hall's experiments adduced in proof, that the ſtony character is producible by fuſion.

An ingenious naturaliſt, after deſcribing a block of baſaltes, in which he diſcovered ſuch appearances as inclined him to admit

admit its igneous confolidation, rejects that hypothefis, merely from the imaginary inability of fire to give to any fubftance a ftony character: " Quelque mélange," fays he, " de terres que l'on fuppofe, quelque foit le tems que l'on emploie, il eft três certain que l'on n'obtiendra pas, par la feul fluide igné, ni bafalte, ni rien que lui refemble."

Sir James Hall's experiments have completely demonftrated the contrary of what is here afferted; they have added much to the evidence of the Huttonian fyftem; and independently of all theory, have narrowed the circle of prejudice and error."

It is anfwered, that Sir James Hall's chryftallite and the original ftone are different.

Sir James Hall, however, has only fhown, that when bafalt, grunftone, or wacken, are completely melted and then flowly cooled, the glaffy character difappears, and the mafs affumes that of indiftinct chryftallization. This chryftallized mafs he denominates chriftalite, as bearing a moft ftriking refemblance to the natural ftones. To the oryctognoft, however, the glafs and the indiftinctly chryftallized mafs are equally different from the original ftone, therefore the arguments drawn from thefe experiments, in favour of the igneous origin of the rocks of the trapp-formation, are unfounded, and the objection which has been now ftated remains in full force.

Objections to Sir James Hall's opinion, that lava has been in a ftate of complete fufion, &c.

Sir James Hall's opinion, that lava has been in a ftate of complete fufion, and by flow cooling has acquired its prefent character, is probably incorrect. The well known facts, that lava has run into the fea, rivers, and lakes, without affuming the glaffy character; that iflands compofed of cellular lava have been formed in the fea; and laftly, the low degree of heat poffeffed by lava ftreams, fhew, that it has not been in a ftate of complete fufion; on the contrary, the following facts, which are to be found in the beft writers on volcanos, render the Wernerian opinion, viz. that the fluidity of lava has rarely exceeded the ftate of vifcidity, extremely probable.

Wernerian opinion, that lava is feldom more than vifcid; fupported by its flow defcent,

1. Lava flows very flowly, even over confiderably inclined planes.

2. In the European volcanos glafs has hardly ever been obferved. The Obfidian of Iceland, as defcribed by Bergman and Olafsen; that of Lipari; the glafs of Vefuvius mentioned by Brierlac, are moft diftinctly products of water, as can be demonftrated by the Wernerian geognofie.

and the fcarcity of glaffy matter, together with

3. The chryftals of hornblende and felfpar, which are fo frequently found in lava, being wrapped up in it (eingewickelt), prove,

prove, as Werner has well obferved, that thefe are the unalter-ed chryftals of the mother-ftone, which the volcanic fire has not had power to melt. the unaltered chryftals it contains.

4. When porphiritic bafalt is brought to the ftate of vifcidity, the chryftals of felfpar float in it unaltered, and the mafs which is now fufficiently liquid to flow, however quickly cooled, does not affume the glaffy character, but has the character of the commoneft kinds of lava. Quick cooling of porphyritic bafalt from its vifcid ftate does not afford the glaffy character.

ROBERT JAMESON.

VII.

Defcription of the Blow-Pipe Apparatus of HAAS. *From a Correfpondent.*

To Mr. NICHOLSON.

DEAR SIR,

AFTER I had communicated to you the apparatus for the blow-pipe by means of alcohol, as ufed by Profeffor Pictet, which you favoured with a place in the laft number of your excellent Journal, I was ftruck with the idea that the apparatus of Mr. Haas, which I mentioned in my letter, though made in the midft of London, is almoft entirely unknown in this country. As this laft is very ingenious, and affords many advantages in the ufe of the blow-pipe, I think you will render a fervice to your chemical readers by making them acquainted with its nature and conftruction. In confequence of this, I here fubjoin a defcription and drawing of it, given by Mr. Orftein in Dr. Scherer's Chemical Journal, Vol. II. page 454. I leave it to your difcretion whether you think it deferving a place, and continue with the higheft efteem, Letter.

SIR,

Your moft humble
and obedient,

N. N.

P. S. Concerning the older conftructions of the blow-pipe, Dr. Scherer refers in that paper to Bergman's Opufc. II. Engftröm's Writers on the blow-pipe.

Engſtröm's deſcription of a mineralogical pocket laboratory (which may be ſeen in the ſecond volume of Magellan's tranſlation of Cronſted's Mineralogy, 8vo, Lond. 1788), and to Weigel's Verſuch einer Geſchichte Eſſay towards a hiſtory of the blow-pipe, and its uſe, in Crell's addition to the Chemical Annals, Vol. IV. page 262—283, and page 393—419; alſo, Vol. V. page 6—24, and page 198—226.

Deſcription of the apparatus.

THE delineation here added is upon the whole ſo diſtinct, that a few parts only require a detailed deſcription. I begin with the candle tube, as it is the moſt eſſential part of this apparatus, on which all the others in ſome manner depend.

Plate VII. A is this candle-tube, which may be ſlided backwards and forwards upon the plate B. The rack in the tube ſerves to raiſe the candle. The ball *s*, together with its tubes *c c*, may alſo be raiſed and lowered in order to direct the point *e* accurately towards the flame. The poſſeſſors of this apparatus will undoubtedly thank Mr. Haas for the ingenious arrangement of the inner parts of the ball, by means of which no moiſture can enter into the point *e*.

The charcoal-ſtand C is likewiſe moveable upon B; but at the ſame time, as the drawing ſhews, its pillars *k k* are likewiſe, by means of the ſliding-pieces *i i*, capable of a lateral motion; beſides that at *o* the coal can be vertically turned upon the pillars.

Peculiar conſtruction of ſnuffers.

The conſtruction of the ſnuffers, and the rectangular ſituation of their point *m* with their flaps to the handle *n*, will be found particularly important and uſeful. For, by reaſon of this form, the wick can, without any inconvenience to the hand, be always cut off in the ſame direction in which the flame is blown, and by ſtroking the wick with the point *m* (turned towards the body of the operator), it may be ſo managed as to be always kept broader in the direction of the flame. This will be found more expedient than the uſual method of bending the wick according to the flame's direction. The handle is laid hold on with the thumb and middle finger at *r r*. The fore-finger, for the purpoſe of ſnuffing the candle, opens or ſhuts the moveable flap by puſhing the piece V to the right or left. It is becauſe a common pair of ſnuffers, in the uſe of this apparatus, would be not only extremely incommodious but even detrimental, that Mr. Haas invented this commodious and uſeful inſtrument.

The

The following articles which make this apparatus complete for ufe, were fold by Mr. Haas * in a neat box: 1. Various candles; 2. Points of different bores; 3. A hammer; 4. A round excavated piece of fteel to pound the products of the operation; 5. A pair of tongs; 6. A file; 7. A knife; 8. A magnifying lens; 9. A fpoon made of platina; 10. A fteel to ftrike fire, ferving at the fame time as a magnet; 11. A quadrangular magnet bar; 12. A thermometer; 13. An hydrometer. Additional apparatus.

VIII.

Note refpecting the Inftruments by which the two Kinds of Electricity are diftinguifhed, or its Direction afcertained. W. N.

CONCERNING the phenomena denominated electric, the facts have hitherto afforded us little more than a few general laws, which certain ingenious philofophers have connected by thofe hypothefes which conftitute the theory. Among thefe the two principal are, 1*ft*, that a peculiar fluid, called the electric matter, exifts in or upon all bodies; capable of being accumulated by friction, eafily and moft rapidly paffing through or along metals, water, and charcoal, and difficultly or not at all through glafs and other bodies not containing fluid water; capable of exifting at the oppofite confines of non-conductors, fo as to be in excefs or plus at one furface, and in defect or minus at the other, in much higher degrees than either ftate could fingly exift; exhibiting the phenomena of ignition and found when it paffes through the fubftance of non-conductors; and capable by its rapid motion of raifing the temperature of conductors to the higheft intenfity hitherto obferved, as well as of exciting mufcular action in all degrees, to the entire deftruction of animal life. Electricity is yet very imperfectly underftood. Franklin's theory of plus and minus.

2. The fecond received theory of electricity admits the exiftence of two fluids capable of being feparated by friction; having the fame habitudes, as to conductors and non-conduc- Nollet's theory of two fluids.

* Mr. Haas is no longer in London: Five fets of this apparatus were fent to Portugal, where he is now eftablifhed. The blowpipe and reft of the adjutage are made of brafs, except the fnuffers. —Tranfl.

tors,

tors, as the ſimple fluid; ſtrongly attracting each other, and not perceptible when combined in a due proportion; capable of exiſting ſeparately at the confines of non-conductors in very high condenſation; cauſing light and ſound when they ruſh together through the ſubſtance of a conductor; and producing the higheſt temperature when they paſs through or meet in non-conductors, &c.

According to the theory of plus and minus, that inſtrument which ſhews the kind will alſo diſtinguiſh the courſe of electricity.

Without entering farther into theſe theories, or the additions they require in order to ſatisfy the facts, ſuch as the attraction ſuppoſed to exiſt between conductors and the electric matter, the atmoſpheres of electricity, &c. the ſubject of the preſent notice requires we ſhould uſe the language of one of the two. The former is moſt commonly adopted; and accordingly we ſhould ſay that an inſtrument which ſhall diſtinguiſh the plus from the minus ſtate of electricity, will alſo ſhew the current or direction in which that matter is moved or carried.

Propoſed means of doing this;

The ſupporters of the theory called Franklin's, from the name of the philoſopher who invented or at leaſt explained it at an early period, have always been aware of the advantage it would be to their ſyſtem if they could adduce any experiment by which the direction of their electric fluid might be aſcertained. Dr. Franklin himſelf very modeſtly offers conjectural reaſons, why the one kind of electricity ſeemed to be a redundancy and the other a deficiency, chiefly grounded on the luminous appearances at the extremity of the wires in the electric ſtates. Henly likewiſe obſerved the lights of the two electricities in the figure of the ſpark, of which the ſtem is always at the plus conductor and the ramifications at the negative; ſo that the forked extremity of lightning will always denote the receiving body, whether it be the cloud or the earth. He alſo uſed his exhauſted glaſs tube with good ſucceſs, as an inſtrument for the ſame purpoſe in which a receiving ball has a luminous atmoſphere, and a giving ball throws out ſtreams of light. The ſame philoſopher firſt noted, that the flame of a ſmall candle is blown towards a negative conductor, and from a poſitive one; though this experiment is not quite unequivocal: And he made ſeveral other experiments which cannot with conſiſtent brevity be here detailed, having this leading object in view.

from electric light; figure of the ſpark; Lightning; exhauſted tube; flame of a candle;

None of theſe afford any deciſion

Among the experiments made with this view, every one of which, it muſt be confeſſed, leave the great queſtion of the nature

nature and direction of the ſuppoſed electric matter undetermined, there ſeems to be only two which afford the ſimple apparatus we are in the habit of diſtinguiſhing by the name of inſtruments. As both inſtruments appear to deſerve more immediate notice than they have yet had, I have inſerted them in Plate VIII, where Fig. 4 repreſents the Galvanic tube, which, by the ſkilful management of Dr. Wollaſton, is rendered capable of diſtinguiſhing the courſe of electricity. The ingenious Mr. Acum, of Compton Street, Soho, who ſupplies chemiſts and philoſophers with apparatus and materials of every kind, made the inſtrument where this drawing was taken. Two fine gold wires are fixed in ſeparate ſmall glaſs tubes by fuſion; and the ends of theſe tubes are ground away till the very ſection or extremity of each wire is laid bare: at the other end of each tube appears a larger wire with a ball, which wire communicates with the gold wire within. Theſe two tubes are fixed by fuſion in a larger tube, provided with a conical drawn aperture, through which it can be filled with water, all but a ſmall vacuity or bubble, and then ſealed. The apparatus is then complete; and if a ſtream of electricity or Galvaniſm be paſſed through it, the water is decompoſed, and of the gas thus extricated, the larger ſtream of hidrogen ſhews the minus ſide. It is probable this inſtrument might be of uſe in atmoſpherical obſervations, in caſe large ſtreams of electricity ſhould paſs through an elevated conductor at a low intenſity.

of the real nature and courſe of electricity.

Two inſtruments for the ordinary purpoſe; viz. the Galvanic tube;

commodiouſly formed;

The other inſtrument, Fig. 5, was conſtructed by myſelf about twenty years ago, from an obſervation I made that low points ceaſe to act as ſuch with the plus at higher elevations, than with the minus electricity. So that if two balls be inſulated, and a low point proceeds from one of them, electricity will fly through the interval in a ſpark if the point be plus, but will be ſilently tranſmitted if the point be minus.

— and an apparatus with a low point.

This inſtrument, like the other, may be applied to diſtinguiſh the atmoſpheric changes; but it will be effectual only when the electricity is ſtrong enongh to give ſparks.

Section

IX.

Section and Plan of a moveable Crane, capable of heaving four Tons with four Men. Designed and executed for the Pier at Ramsgate; by Order of the Trustees for the Management of the Harbour at that Place. By Mr. PETER KEIR, *of St. Pancras. Communicated by Mr.* P. KEIR.

To Mr. NICHOLSON.

SIR,

Account and description of the crane.

THE crane represented in the annexed drawings was constructed at the request of the Ramsgate Harbour Trust. It consists of a cast iron frame, marked A, Plates V. and VI. weight two tons, and 9 feet 7 inches square, supported on four cast iron wheels, two of which traverse on a centre at B, by the rack work C, to steer it when moving from place to place. A cast iron upright shaft, D, weight 23cwt. supported by oak braces; the weight of the framing and wheel work is supported by a steeled pivot on the top of the shaft at F, round which the whole swings, including the men that work it.

The drawing is sufficiently plain, to require no farther explanation. It has been executed, and may be inspected at any time on Ramsgate Pier.

I am, SIR,

Your obedient humble servant,

PETER KEIR.

Further particulars.

Upon inquiry, I find that the original instructions were to make a crane capable of raising four tons, and that the centre piece, or upright shaft, was at first made of oak: but that the strain for disengaging goods from their stowage requiring the occasional exertion of a greater power, it was found adviseable to substitute the present iron piece. The cost of this engine was four hundred pounds; but from the above circumstance, and particularly because the expence of patterns, &c. will not again be requisite, I am informed that others might now be made for three hundred pounds each.

W. N.

Experiments

X.

Experiments on the Separation of Light and Heat by Refraction. In a Letter from Sir H. C. ENGLEFIELD, *Bart. F. R. S. to Thomas Young, M. D. F. R. S. From the Journal of the Royal Institution, p.* 202.

DEAR SIR,

IN pursuance of your desire, I communicate to you the experiments which I made in consequence of Dr. Herschel's most interesting discovery of the separation of solar heat and light by the prism. They carried with them, to my mind, a complete conviction of the truth and accuracy of the Doctor's assertions on that subject. Should you judge them worthy of insertion in the Journals of the Royal Institution, I shall think myself honoured by their publication. **Letter to Dr. Young.**

I am, Dear Sir,
Your faithful Servant,
H. C. ENGLEFIELD.

As I was desirous not only to ascertain, by actual observation, the very curious discovery of Dr. Herschel, but to obviate the objection made by Mr. Leslie * to the mode in which his experiments had been made, I contrived an apparatus totally different in its arrangement from that which had been used by the Doctor; and disposed in such a manner that it was impossible that the thermometers should be affected by any foreign heat reflected from any part of it. As to the heat reflected from the floor, it could not influence the results, since it was the same, whatever colour was thrown on the ball of the thermometer. **Introductory remarks.**

As I had nothing to do with light, it was not necessary to darken the room; and as I wished to accumulate as large a portion of solar heat as possible, I placed the prism in an open window, instead of admitting a beam through an aperture in a shutter, as is the usual practice. The prism I used is a very good one; and it was lent to me for this purpose by Mr. Walker, of Conduit-Street. It is three inches long, and equilateral, each side being 1.15 inch broad. **A prism was placed in the solar light in a room not darkened.**

* In his communications to the Philosophic Journal (quarto) Vol. IV. p. 344, 416.

It

It was fupported by an horizontal arm, projecting from a pole like that of a fire-fcreen. The arm could be adjufted to any height by a fcrew; and the prifm was likewife capable of being turned on its axis to any required pofition.

The fpectrum fell on a lens.

The coloured fpectrum was thrown on a very good lens, of four inches in diameter, and about 22 inches focal length. This lens formed part of a machine well known, and ufed for viewing prints; it ftands on a foot adjuftable in any direction, and to any height; and the mounting being of wood, and merely fufficient to fupport the lens, fcarcely any heat can be accumulated in any part of it. The whole lens, as well as its mounting, was covered with a thick white pafteboard fcreen, in which was cut a flit of 3 inches long, and half an inch wide; this flit was over the centre of the lens; and through it any one of the colours required was admitted on the lens, while all the reft of the fpectrum was totally excluded. A light wooden arm, of two feet long, projected at right angles from the lower part of the mounting of the lens. This arm carried a fmall fcreen of polifhed card, which received the image formed in the focus of the lens. This was found neceffary, in order to find with certainty where to place the thermometer; when the focal diftance was found, the fcreen was moved back, about a diameter of the bulb of the thermometer ufed, which was then held by the hand in the focus of the lens, which was done with great eafe and certainty, as nothing more was neceffary than to fix the eye on the card fcreen, and keep the ball of the thermometer in the centre of the luminous image. The whitenefs and polifh of the fcreen totally precluded any accumulation of heat in it; nor indeed would fuch accumulation have been of any detriment to the experiments had it exifted; for, as it muft have been entirely owing to the ray under examination, it would have done nothing more than increafe the effect of it on the thermometer.

Through a flit which fuffered one colour only to pafs.

A mercurial thermometer was held in the focus.

The thermometers ufed were mercurial, and very fenfible. The fcales were ivory tubes, embracing the ftem, and graduated within. The ball of the inftrument was therefore unconnected with any mounting; and no falfe heat could poffibly affect it. The balls were moftly blackened with Indian ink, carefully laid on; but fome were ufed naked, and one, covered with white water-colour paint.

The

Poſitions of the lens and priſm.

The lens, with its apparatus, was placed about three feet from the priſm; and, as the ſun was pretty high in the greater part of the experiments, and the deſcending ſpectrum was in general uſed, the floor under the ſtand of the lens was in ſhade, from the wall below the window, and had been ſo all the day, as the window at which moſt of the experiments were made, fronted the ſouth

Some part of this detail might ſeem ſuperfluous, had not ſuch ſtreſs been laid on the ſuppoſed accumulation of heat in Dr. Herſchel's experiments, that it was neceſſary to obviate any objection on that head; or to ſhow, at leaſt, the utter improbability of any cauſe of that nature having affected the reſults of theſe experiments.

I now proceed to relate the experiments themſelves: which I ſhall tranſcribe from the original notes made at the moment.

April 6, 1801.

Experiment I.

Maximum riſe of temperature of the therm.

in blue	= 1°
green	= 4
yellow	= 6
red	= 16
conf. r.	= 15½
beyond	= 18

The apparatus being diſpoſed as above deſcribed, the coloured rays of the deſcending ſpectrum of the priſm were ſucceſſively thrown on the ſlit in the ſcreen, covering the lens; and the thermometer with a blackened ball, placed in the focus of the lens, roſe as follows:

In the blue in 3′, from 55° to 56°. or 1°.

Green in 3′, from 54° to 58°. or 4°.

Yellow in 3′, from 56° to 62°. or 6°.

Full red in 2½′, from 56° to 72°. or 16°.

In the confines of the red in 2½′, from 58° to 73½°. hr 15½°.

Quite out of viſible light in 2½′, from 61° to 79°. or 18°.

Between each of the obſervations the thermometer was placed in the ſhade ſo long as to ſink it below the heat to which it had riſen in the preceding obſervation: of courſe its riſe above that point could only be the effect of the ray to which it was now expoſed *. A thermometer placed conſtantly in the ſhade near the apparatus, ſcarcely varied during the experiments.

April 17th, 11, A. M.

Three thermometers, uſed afterwards in the experiments, were expoſed to the ſun's rays until they became ſtationary.

* In all the experiments the thermometer was continued in the focus long after it had ceaſed to riſe; therefore the heats given are the greateſt effect of the ſeveral rays on the thermometer in each obſervation.

The

The thermometer with

Naked ball	$58\frac{1}{2}°$
Whitened ball	$58\frac{1}{2}°$
Blackened ball	63°

Experiment II. The apparatus being placed all as before, the blackened thermometer

Black ther. rose in red = 3° dark = 5
White therm. in red 3°. dark $0\frac{1}{2}$
Black ther. in dark 4°.

In full red ray in 3′, from 58° to 61°. or 3°.

In quite dark in 3′, from 59° to 64°. or 5°.

The whitened thermometer

In full red ray in 3′, from 55° to 58°. or 3°.

In quite dark in 3′, from 58° to $58\frac{1}{2}°$. or $0\frac{1}{2}°$.

The blackened thermometer was now again placed in the quite dark, and rose in 3′, from 58° to 61°. or 4°.

This is what I expected, as a thick smoky haze had come on since the experiments were begun, and increased much towards the end of them.

Experiment III. April 18, 11, A. M. Sun clear. All the apparatus as before.

Black ther. rose in confine of red 12°
White do. do. 3°

In the confines of the red.

Black thermometer in 3′, from 59° to 71°.

White thermometer in 3′, from $57\frac{1}{2}°$ to $60\frac{1}{2}°$. Clouds came on, and put a stop to the experiments.

Experiment IV. April 19, $3\frac{1}{2}$, P. M. Sun clear.

Black ther. rose in full red 12°
White do. do. $3\frac{1}{2}°$.

In the full red ray.

Black thermometer in 3′, from 66° to 82°

White thermometer in 3′, from 66° to $69\frac{1}{2}°$.

In confine of red
Black th. $12\frac{1}{2}°$
Ditto, quite dark 14°.

In the confines of the red.

Black thermometer in 3′, from 67° to $79\frac{1}{2}°$. But thin streaky clouds had come over the sun.

In quite dark, half an inch out of the red.

Black thermometer in 3′, from 70° to 84°.

Transitions from red to dark occasioned changes of temperature.

When the thermometer was carried into the faint red light, it sunk quickly; and rose again as quickly when carried into the dark focus: but when carried into the dark on the other side of the red light, it sunk very rapidly, and did not appear to receive any heat at all. Thin clouds increased, and rendered the sun's light too faint for further experiments.

Experiment V. April 20, from $10\frac{1}{2}$ to $11\frac{1}{2}$, A. M. Sun quite clear.

Although it could not be supposed that effects of the refracted light could differ in the two spectra, yet, in order to ascertain the fact, the horizontal spectrum was used in the subsequent experiments.

The

The apparatus all the ſame as in the former.

In the full red ray.

Black therm. full red $4^\circ\frac{1}{2}$ dark $9\frac{1}{2}^\circ$.

Black thermometer in 3′, from 67° to $71\frac{1}{2}°$

Quite out of the ray.

Black thermometer in 3′, from 68° to $77\frac{1}{2}°$.

Darker $10\frac{1}{2}°$.

The ray was now ſo far removed from the ſlit in the ſcreen, that ſcarce any light was perceptible in the focus of the lens. The black thermometer was now placed near half an inch from the bound of the viſible light in the focus, and roſe in 3′, from 69° to $79\frac{1}{2}°$.

Moſt dark 9°.

The utmoſt edge of the priſmatic ſpectrum was now removed an eighth of an inch from the edge of the ſlit in the ſcreen; and no light was now viſible in the focus of the lens.

Black thermometer in 3′, from 70° to 79°.

The difference perceivable by the touch.

Mr. Cary, optician in the Strand, and Dr. Hunter, were preſent at theſe experiments, and repeatedly ſaw the thermometer, in the ſecond experiment, ſink when carried into the light, and riſe again when removed back into the dark. Dr. Hunter alſo received the focus on the palm of his hand, where the heat was ſenſibly felt; and on ſhutting his eyes, and pointing with a long pen to where the heat was greateſt, he always touched his hand beyond the viſible light.

Deſcription of the red image.

As the red image has been continually mentioned in the courſe of the above recited experiments, it may not be improper to deſcribe it more particularly. The diameter of the red ſpot, formed by the ray in the focus of the lens, was juſt two tenths of an inch in diameter, at right angles to the length of the ſpectrum, and well defined: in the direction of the ſpectrum it was elongated, as might be expected, and leſs well defined.

Very faint red in the focus when the heat was greateſt.

When the whole viſible ſpectrum from the priſm was received on the ſcreen which covered the lens, and the utmoſt edge of the red rays was removed a full eighth of an inch from the edge of the ſlit in the ſcreen, there was ſtill a faint bluſh of red, of a ſemioval form, viſible when the focus of the lens was thrown on a white ſcreen; and it was in theſe circumſtances that the greateſt effort of heat was conſtantly produced on the thermometer, not by placing it in the red light, but out of it, in the axis of the lens.

Repetition of experiments.

I have only to add, that in the courſe of the month of June 1802, I repeated moſt of theſe experiments with the ſame

 apparatus,

apparatus, in preſence of Mr. Davy, with the moſt complete ſucceſs; the ſun's altitude being greater, the effect of his rays was ſo great as to raiſe the thermometer in the inviſible ray to 98°, while the viſible red never raiſed it above 87°. At the ſuggeſtion of Mr. Davy, we tried ſeveral experiments with reſpect to the power of the ſeveral coloured rays in rendering Canton's phoſphorus luminous; and we found, without a poſſibility of doubt, that the blue rays poſſeſſed that power in a much higher degree than the red. There was great reaſon to ſuſpect that this power, like that of blackening the nitrate of ſilver, extended beyond the viſible blue ray; but our apparatus was not prepared for the more delicate part of theſe experiments, which are only mentioned with a view of exciting further reſearches on this very intereſting ſubject, and of giving to Mr. Davy the credit due to him for having firſt thought of the experiment.

The blue rays illuminate Canton's phoſphorus more than the red

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XI.

*On the Expanſion of the Elaſtic Fluids by Heat.** *By* Mr. John Dalton.

Occaſion of the Experiments and Eſſay.

THE principal occaſion of this eſſay is another on the ſame ſubject by Meſſrs. de Morveau and du Vernois in the firſt vol. of the Annales de Chimie. It appearing to them that the reſults of the experiments of De Luc, Col. Roi, de Sauſſure, Prieſtley, Vandermonde, Berthollet and Monge did not ſufficiently accord with each other; and that it would be of importance to determine not only the whole expanſion of each gas from two diſtant points, ſuch as the freezing and boiling, but likewiſe whether that expanſion be uniform in every part of the ſcale, they inſtituted a ſet of experiments expreſsly for thoſe purpoſes. The reſult of which was, that betwixt the temperatures of 32° and 212°, the whole expanſion of one gas differs much from that of another, it being in one inſtance about $\frac{4}{10}$ of the original, and in others more than 12 times that expanſion; and that the expanſion is much more for a given number of degrees in the higher than in the lower

Experiments of Guiton and Du Vernois.

* Mancheſter Memoirs, V. 595.

 part

part of the ſcale. Theſe concluſions were ſo extremely diſcordant with and even contradictory to thoſe of others, that I could not but ſuſpect ſome great fallacy in them, and found it in reality to be the fact: I have no doubt it aroſe from the want of due care to keep the apparatus and materials free from moiſture.

My method of experimenting on this ſubject is ſimple, and therefore leſs liable to error. A ſtraight manometer tube, ſuch as has been mentioned, is duly divided into equal portions of capacity; it is then dried by a wire and thread, and the open end inſerted through a cork into a phial containing ſulphuric acid, in order that the aqueous vapour may be drawn out of the tube; this is eſſential if we operate in temperatures lower than that of the atmoſphere, otherwiſe not. For want of this attention, Col. Roi, in his valuable paper in the Philos. Tranſ. vol. 67, has been led into ſome erroneous concluſions. A ſmall column of dry mercury is then let down to a proper point in the manometer, and it is ready for experiment with common air.

The author's method with a ſimple manometer tube.

and air dried by ſulph. acid.

It requires ſome addreſs to fill the manometer with any other gas.—I ſucceeded beſt as follows: filled the tube with dry mercury; then puſhed down a wire with thread, ſo that when the wire was got to the end of the tube, a thick covering of thread juſt entered the open end, and held the mercury like a cork, ſo that the tube could be inverted without loſing the contents; then having a glaſs funnel with a perforated cork over the water apparatus, containing the gas, I ſlipped the manometer through the hole in the cork, and putting my hand into the water under the funnel, drew the wire out of the manometer, and with it the mercury; upon which the gas entered the manometer. For carbonic acid gas, I opened the ſealed end of the manometer, drew it out to a capillary bore, and forced a ſtream of the gas through the tube; then putting my finger on the other end, ſealed it again by a blow-pipe, and let down a ſmall column of mercury to the proper point.

Method of filling the manometer.

Carbonic acid gas.

When the manometer was to be expoſed to a heat of 212°, I uſed a Florence flaſk, with a long glaſs tube corked into it, in ſuch ſort that as much of the manometer as was neceſſary to be expoſed to the temperature might be in the tube; then the water at the bottom of the flaſk was made to boil violently, ſo that a conſtant ſtream of vapour iſſued out of the top of

Simple method of applying the boiling heat.

the glafs tube, which was found to raife the thermometer to 212°. Small fpecks of white paint were put upon the divifions of the manometer together with numbers which were difcernible through the containing tube. For lower temperatures, a deep tin veffel containing hot water was ufed, in which the manometer was immerfed, the water being well agitated previoufly to each obfervation.

The conclufions of De Luc, Roy, Berthollet, &c. are accurate; but thofe of Morveau and du Vernois not fo.

From a great many experiments made in this way on common air, and likewife upon hydrogenous gas, oxygenous and nitrous gafes, and carbonic acid gas, I can affert that the conclufions of De Luc, Roi, Sauffure, Berthollet, &c. are nearly accurate throughout, and that thofe of de Morveau and du Vernois are extremely inaccurate in the higher temperatures.

Expanfion of common air.

I have repeatedly found that 1000 parts of common air of the temperature 55° and common preffure, expand to 1321 parts of the manometer; to which adding 4 parts for the correfponding expanfion of glafs, we have 325 parts increafe upon 1000 from 55° to 212°; or for 157° of the thermometric fcale.

Intermediate expanfion.

As for the expanfion in the intermediate degrees, which Col. Roi's experiments fhew to be a *flowly diminifhing* one above the temperature of 57°, but which de Morveau's on the contrary fhew to be a *rapidly increafing* one in the higher part of the fcale; I am obliged to allow that Col. Roi is right, though it makes in fome degree againft an hypothefis I have formed relative to the fubject; he has certainly however made the diminution too great from 72° downwards, owing to his not perceiving that he actually *deftroyed* a portion of the elaftic fluid he was operating upon (aqueous vapour) in reducing its temperature fo low; if his air had been previoufly dried by fulphuric acid, &c. he would not have found fo remarkable diminution below 72°. My experiments give for $77\frac{1}{2}$° above 55°, 167 parts; for the next $77\frac{1}{2}$° only 158 parts; and the expanfion in every part of the fcale feems to be a gradually diminifhing one in afcending.

Other gafes agree with common air.

The refults of feveral experiments made upon hydrogenous gas, oxygenous gas, carbonic acid gas and nitrous gas, which were all the kinds I tried, agreed with thofe on common air not only in the total expanfion, but in the gradual diminution of it in afcending: the fmall differences obferved never exceeded 6 or 8 parts on the whole 325; and differences to

this

this amount will take place in common air, when not freed from aqueous vapour, which was the ſituation of all my factitious gaſes.

The law is general for all elaſtic fluids.

Upon the whole therefore I ſee no ſufficient reaſon why we may not conclude, that *all elaſtic fluids under the ſame preſſure expand equally by heat,*—and that *for any given expanſion of mercury, the correſponding expanſion of air is proportionally ſomething leſs, the higher the temperature.*

Suppoſed cauſe they have to counteracting attraction.

This remarkable fact that all elaſtic fluids expand the ſame quantity in the ſame circumſtances, plainly ſhews that the expanſion depends *ſolely* upon heat: whereas the expanſion in ſolid and liquid bodies ſeems to depend upon an adjuſtment of the two oppoſite forces of heat and chemical affinity, the one a *conſtant* force in the ſame temperature, the other a *variable* one, according to the nature of the body; hence the unequal expanſion of ſuch bodies. It ſeems therefore that general laws reſpecting the abſolute quantity and the nature of heat, are more likely to be derived from elaſtic fluids than from other ſubſtances.

Hypotheſis to explain the law of expanſion.

In order to explain the manner in which elaſtic fluids expand by heat, let us aſſume an hypotheſis that the repulſive force of each particle is exactly proportional to the whole quantity of heat combined with it, or in other words to its temperature reckoned from the point of total privation: then, ſince the diameter of each particle's ſphere of influence is as the cube root of the ſpace occupied by the maſs, we ſhall have $\sqrt[3]{1000} : \sqrt[3]{1325}$ (10 : 11, nearly) :: the abſolute quantity of heat in air of 55°: the abſolute quantity in air of 212°. This gives the point of total privation of heat, or abſolute cold, at 1547° below the point at which water freezes. Dr. Crawford (On Animal Heat, &c. page 267) deduces the ſaid point by a method wholly different to be 1532°.—So near a coincidence is certainly more than fortuitous.

Singular coincidence as to the abſolute zero.

An objection if we admit the expanſion of mercury to be as the heat; but not ſo if mercury follow the law of all other fluids.

The only objection I ſee to this hypotheſis is, that it neceſſarily requires the augmentation of elaſtic fluids for a given quantity of heat to be greater in the higher temperatures than in the lower, becauſe the cubes of a ſeries of numbers in arithmetical progreſſion differ more the larger the numbers or roots: but it has juſt been ſhewn that in fact an augmentation of a contrary kind is obſerved. This refers us to the conſider-

ſideration whether the mercurial thermometer is an accurate meaſure of the increments of heat; if it be, the hypotheſis fails; but if equal increments of heat cauſe a greater expanſion in mercury in the higher than in the lower temperatures, and that in a ſmall degree, the fact noticed above inſtead of being an objection will corroborate the hypotheſis.—Dr. Crawford determines the expanſions of mercury to be very nearly in proportion to the increments of heat: M. De Luc makes them to be leſs for a given quantity of heat in the lower than in the higher part of the ſcale; and in a ratio that agrees with this hypotheſis. Now as every other liquid we are acquainted with is found to expand more in the higher than in the lower temperatures; analogy is in favour of the concluſions of De Luc, that mercury does the ſame.

Short account of ſimilar reſearches by Mr. Gay Luſſac.

In the Bulletin des Sciences, there is a notice of a memoir of Cit. Gay Luſſac on the dilation of the gaſes and vapours; and the memoir itſelf is given in the 43d volume of the Annals de Chémie, whence I ſhall, as early as convenient, give either a full abſtract or a tranſlation. This able author gives the ſame concluſion as that of Mr. Dalton, and, like him, attributes the errors of preceding experimenters to the humidity of the gaſes they examined.

His method.

His experiments were made by expelling the gas from a glaſs ball properly fitted up, and determining the expanſion by weighing the apparatus, after water had been ſuffered to come in during the cooling, to occupy the ſpace of the extruded gas. He very candidly informs us, that Cit. Charles had deduced the general reſult of the equal and ſimilar expanſion of all the gaſes by heat, under like preſſure, fifteen years before, though he did not publiſh it; and he gives an hiſtorical account of the labours of his predeceſſors; ſo that we muſt neceſſarily conclude that he could not have ſeen the late publication of the Mancheſter Society, in which the extenſive reſearches of Mr. Dalton are inſerted; and wherein it appears that the preceding memoir was read about a year before that of Mr. Gay Luſſac was read to the Inſtitute. The French Philoſopher finds the expanſion or increaſe of dimenſion, between the freezing and boiling water points, to be 0. 375,

Mr. Dalton's experiments were earlier.

which

which gives for the ſpace between 55° and 212°, or 157°, the quantity 0.327 inſtead of 0.321, as Mr. Dalton finds it. This difference of ſomewhat leſs than one fiftieth part of the expanſion, may perhaps have ariſen from a difference in the ſcales of operation, of perhaps the difficulty of producing a given temperature through the whole maſs of a fluid, &c.

How far they agree.

XII.

Account of ſome Experiments made in the Laboratory of the Royal Inſtitution, relating to the Agencies of Galvanic Electricity, in producing Heat, and in effecting Changes in different fluid Subſtanees. By H. DAVY, *Prof. Chem.*

I. IT has been ſhown, by a very intereſting experiment made in France, by Meſſrs. Fourcroy, Vauquelin, and Thenard, that the power of galvanic batteries containing large plates, to ignite metallic ſubſtances, is much greater than that of batteries compoſed of an equal number of ſmall plates; though their agencies upon water, and upon the human body, are nearly the ſame.

Diſcovery of Fourcroy, Vauquelin, and Thenard, of the greater ignition by large galvanic plates.

In examining the circumſtances of the action of a galvanic apparatus, or trough, conſtructed in the Royal Inſtitution, and containing twenty ſeries of plates of copper and zinc, ſquare, and thirteen inches in diameter, I obſerved that the ſame relations between chemical agency and the production of galvanic electricity exiſted as in other caſes. When pure water was uſed for filling the cells, the ſparks, as well as the ſhocks, were extremely indiſtinct; and the battery was capable of igniting only about a line of iron wire of $\frac{1}{170}$ of an inch in diameter. With ſolution of muriate of ſoda it acted better; and diluted nitric acid was ſtill more efficacious. With this laſt ſubſtance, it became capable of rendering white-hot three inches of the iron wire of $\frac{1}{170}$, and of cauſing two inches to enter into fuſion.

The trough with large plates has the ſame relation of chemical agency as other apparatus.

Water had little effect; ſalts and acid much more.

In comparing the effects produced by a ſolution of nitrous acid, of the ſpecific gravity of 1.4, in about ſixty parts of water, with thoſe occaſioned by a concentrated ſolution of carbonate of potaſh, the acid was found to produce by much the greateſt intenſity of action; which can hardly be aſcribed to any

Nitrous acid did not act *by its conducting power*. For it is much *more* powerful than carbonate of potaſh, though it conducts much *leſs*.

any other cauſe than its chemical agency; for, with regard to conducting power, it appeared very much inferior to the other ſolution. There is every reaſon to believe, that with pure water, that is, water deprived of air and of all ſaline ſubſtances, no action would be produced in this battery. I was unable to aſcertain the fact by a direct experiment; but I found repeatedly, that a pile, compoſed of thirty-ſix ſeries of ſquare plates, of copper and zinc, of five inches in diameter, loſt its activity in nitrogene and hydrogene gaſes, in about two days; and it was conſtantly reſtored by common air; and rendered more intenſe by oxigene gas.

Water appears to act, not as a conductor, but by its gas.

Water and oil made to boil by galvaniſm.

II. When the galvanic battery, with large plates, was in full action, it was found that a w $\frac{1}{80}$ of an inch in diameter, and two feet long, when placed in the circuit, was rendered ſo hot as to cauſe a ſmall quantity of water, brought in contact with it, ſpeedily to boil. It continued warm for many minutes; and, by an occaſional momentary interruption and completion of the circle, the heat was permanently kept up. When three or four inches of the wire of $\frac{1}{170}$ were placed in any part of the conducting chain, they continued red-hot for more than a minute; and, by a ſucceſſion of interruptions and contacts, they were kept partially ignited for five or ſix minutes. When that part of the communicating chain containing the ſmall wire was introduced into a ſmall quantity of ether, alcohol, or oil, the fluid ſoon became warm; and olive oil, the only ſubſtance that was expoſed for a ſufficient time, was made to boil,

Charcoal made red hot under water, oils, &c. with the extrication of gaſes.

III. When two ſmall pieces of well burned charcoal, or a piece of charcoal and a metallic wire, were made to complete the circle, in water, vivid ſparks were perceived, gas was given out very plentifully, and the points of the charcoal appeared red-hot in the fluid, for ſome time after the contact was made; and, as long as this appearance exiſted, elaſtic fluid was generated, with the noiſe of ebullition. The *ſenſible* phenomena were nearly the ſame with the volatile and fixed oils, ether, and alcohol; and, by means of charcoal, the ſpark could be produced in concentrated ſulphuric and nitric acids, which are amongſt the beſt of the leſs perfect conductors.

Nature of the gaſes;

The gaſes produced from different fluids by the galvano-electric ſpark, were examined; and as the reſults were, in moſt

moſt caſes, what might have been expected from theory; the analyſis of them was not made with very minute attention.

When water was acted upon by ſparks taken from two pieces of charcoal, the elaſtic products evolved were about $\frac{1}{8}$ of carbonic acid, $\frac{1}{8}$ of oxigene, and the remainder an inflammable gas, which required a little more than half its volume of oxigene for its combuſtion. With gold and charcoal, the gold being on the zinc ſide, the gas produced appeared to be chiefly a mixture of oxigene and hidrogene, for it diminiſhed $\frac{7}{10}$ by the electric ſpark. — from water;

The gas diſengaged from alcohol, the ſpark being taken by gold connected with the zinc end, and charcoal, was a mixture of nearly two parts of oxigene and eleven parts of inflammable gas, which appeared to be partly light hidrocarbonate. — from alcohol;

Ether, in the ſame method of operating, gave four parts of oxigene and twelve parts of inflammable gas. — ether;

From ſulphuric acid, oxigene and hidrogene were produced very rapidly, (the oxigene being more than ſufficient for the ſaturation of the hidrogene by combuſtion,) and the acid became blue. — ſulph. acid;

The gas from nitric acid detonated with great violence by the electric ſpark, and the reſiduum was oxigene, mixed with a little nitrogene. — nitric acid.

The products from the acids, there is every reaſon to believe, were evolved chiefly in conſequence of the decompoſition of the water they contained. And, in operating upon theſe ſubſtances, as well as upon pure water, a portion of the elaſtic fluids muſt have been produced at the time of the ſilent tranſmiſſion of the electricity, during the momentary interruptions of contact. The apparent ignition of the charcoal in the different fluids depended, probably, in ſome meaſure, upon its being ſurrounded, at the moment of contact, by globules of gas, which prevented the heat, produced at the points of it, from being rapidly carried off by the fluid. — Ignition of the charcoal accounted for.

When the ſpark was taken by means of iron wires, in phoſphorus rendered fluid by heat, under a ſtratum of water, permanent gas was produced from it, but in a quantity too ſmall to be examined, after a proceſs that continued an hour. I purpoſe to repeat the experiment, with conductors of dry charcoal. — Gas from fuſed phoſphorus.

IV.

The large plates disengage most gas; provided the conductor's power be good.

IV. When gold wires, connected with the ends of the battery, were made to act upon fluids in the common method of communication, being placed at a distance from each other, it was found that the rapidity of the evolution of the gases was much more influenced by the conducting power of the fluid, than it is in common cases with small plates. In comparing the action of a battery of twenty plates, of five inches in diameter, upon sulphuric acid, nitric acid, and various saline solutions, with that of the large battery, it was observed, in several experiments, that the gas was disengaged much faster, and in larger quantities, from the wires connected with the large plates, whilst the action of the two arrangements upon water was nearly the same. This fact, combined with other facts of the same kind, seems to show, that the quantity of electricity excited in the arrangements with large surfaces, is much greater than that produced in those with small surfaces; and that it is capable of passing with facility through the more perfect conductors, whilst, from the nature of the series, its circulation is impeded, comparatively to a great extent, by imperfect conductors; a conjecture that has been already formed by different philosophers.

Attempt to produce change in muriatic gas, by the galvanic ignition of charcoal.

V. As the great quantity of electricity made to circulate through perfect conductors, by means of the large apparatus, increases their affinity for oxigene more perhaps than any known agent, and as charcoal by means of it can be rendered white-hot, and kept in constant combustion in oxigene gas or atmospherical air, I thought of trying the effects of the electrical ignition of this substance, upon muriatic acid gas confined over mercury.

Experiment.

The experiment was made by means of a small glass tube *, containing a slip of platina hermetically sealed into it, and having a piece of charcoal attached to its lower extremity: the communication was effected by means of iron wires; and the charcoal was made white-hot, by successive contacts continued for nearly two hours. At the end of this time, the muriatic acid gas had diminished a very little in volume: much white matter had formed upon the charcoal, which was not sensibly consumed. When the gas was examined, $\frac{3}{4}$ of it were instantly absorbed by water, and the remainder proved to be

* For a description of this apparatus, see p. 214.

inflam-

inflammable. The procefs was repeated three times; and, when the fpark was moft vivid, a white cloud was always perceived at the moment of its production, I am inclined to attribute this phenomenon, and the other phenomena, to the decompofition of the water held in folution in the gas, by the charcoal and the mercury adhering to it; and the white matter was probably muriate of mercury. The acid gafes are rapidly abforbed by charcoal; and this fubftance, when well made, will take up more than 30 times its volume of muriatic acid gas; fo that, in the procefs of ignition, a part of the water and of the acid muft have been acted upon in a very condenfed ftate. Effects;

The want of fuccefs in this experiment, the refults of which are very fimilar to thofe obtained by Mr. William Henry *, in his trials with common electricity, prevented me from carrying on the procefs upon fluoric acid gas, as I had at firft intended. Many of the compound gafes that are decompofable by heated charcoal, might probably, however, be analyfed in a very fimple manner, by means of the ignition of that fubftance by galvanic electricity; and this mode of operating may be conveniently applied, for afcertaining the relations of the affinities of charcoal for the conftituent parts of compound gafes at very high temperatures. very fimilar to thofe of Mr. W. Henry with common electricity. No decompofition.

SCIENTIFIC NEWS, &c.

NATIONAL INSTITUTE OF FRANCE.

Prize propofed at the Public Sitting of the 15 *Germinal, in the* 10*th Year. (April* 5, 1802.)

THE general conditions are as ufual; namely, that the name of the author fhall be concealed, and his rank diftinguifhed by a fentence or device, which fhall alfo be written on a fealed billet, containing his name and addrefs. The works are to be addreffed (carriage paid) to one of the Secretaries of the Inftitute. Neither the treatifes, nor any draw- Conditions.

* Phil. Tranf. 1800, p. 188, or Philof. Journal, quarto, iv. 209. 245.

ings

ings or machines thus forwarded, will be reſtored; but the authors may take copies, and may again receive their models or machines, upon ſubſtituting regular drawings in place of them. The Commiſſioner of the Funds of the Inſtitute will deliver the gold medal to the bearer of the Secretary's receipt, if given for the treatiſe, &c.; but where no receipt exiſts, the author muſt himſelf appear, or ſend his procuration.

Subject of the Prize of Chemiſtry.

The claſs of mathematical and phyſical ſciences having propoſed in the year 8 the following prize queſtion, to be decided upon at the ſitting above mentioned:

Prize queſtion reſpecting ferments.

What are the characters which diſtinguiſh in vegetable and animal matters thoſe which ſerve as ferments, from thoſe in which they cauſe or excite fermentation.

And none of the memoirs having fulfilled the conditions of the program, the claſs propoſes the ſame ſubject again for the year 12.

The prize will be a gold medal, of the value of one kilogramme, which will be given at the ſitting of the 15 Germinal of the year 12 (April 5, 1804).

Prize Queſtions from the Batavian Society at Haarlem.

Prize queſtions.

The Batavian Society at Haarlem has publiſhed a number of ſubjects of prizes at their ſitting of May 1, 1802; among which are the following:

Reſpecting manures.

1. "How far it is known, from the lateſt diſcoveries in the phyſiology of plants, in what manner the different manures on different ſoils are favourable to the vegetation of plants; and what indications may we deduce from the knowledge required on this ſubject, to direct our choice as to manures, and the fertilization of uncultivated and arid lands?"

This queſtion is continued to the 1ſt of Nov. 1802.

— motion of the ſap.

2. "What is the actual ſtate of our knowledge reſpecting the motion of the ſap in trees and plants? In what manner can we acquire a more complete knowledge reſpecting the obſcure and doubtful parts of the ſubject? And is it poſſible to deduce, by deciſive experiments, ſuch indications as may be uſeful for the cultivation of trees and plants?"

The Society has determined to repeat this queſtion, to which anſwers are to be ſent before November 1, 1803.

3. "The

3. "The Society demands a theory, or phyfical explication, which fhall clearly and diftinctly fhow the caufes of the afcent of fmoke in chimnies, or thofe which prevent its rifing. Together with rules, deduced from this theory, for the conftruction of chimnies, which fhall point out the circumftances neceffary to be attended to in the feveral cafes to prevent fmoke from entering the apartments?" — the afcent of fmoke.

This queftion is here again repeated, and the anfwer is demanded before the 1ft of November, 1803, in order that the authors of memoirs may be enabled to correct them.

4. "What indigenous plants, hitherto not ufed, may, from well confirmed experiments, afford good colours, which may be prepared and ufed with profit? And what exotic plants may be cultivated with profit upon the lefs fertile or uncultivated lands of the Republic, in order to the extraction of colours?" — dying plants.

The Society has determined to repeat this queftion, which is continued without limit of time.

The queftions for the prefent year are the following:

5. "What do we learn, from the lateft obfervations, refpecting the influence of the oxigen of the atmofphere, whether combined or not, together with the action of light upon the changes of colour? And what advantages may be derived from this knowledge?" — effect of oxigen on colours.

The Society defires that it may be fhown concifely, and with precifion, what is well proved by obfervations or experiments, in order that the actual ftate of the fcience with regard to this fubject may be more readily apprehended, and greater advantages obtained in trade, or in the other branches of economy.

The term of concurrence is November 1, 1803.

6. "What light has been thrown upon the manner in which plants acquire their nourifhment, in confequence of the difcoveries refpecting the decompofition of water and atmofpheric air? And what deductions can be made from this knowledge for the improvement of ufeful vegetables?" — nourifhment of plants.

The term of concurrence is November 1, 1803.

7. "What facts are well proved with regard to the purification of corrupted water, and other impure fubftances, by charcoal? How far is it poffible to explain this effect by the principles of chemiftry? And what further advantages may be thence derived?"

2 The

The term of concurrence is November 1, 1803.

The following queftions were propofed formerly, but the term of concurrence is fixed to the firft of November, 1803.

phyfiology of the human body;

1. "What light has the new chemiftry thrown upon the phyfiology of the human body?"

difeafes;

2. "To what extent has this light been of ufe in rendering us better acquainted with the nature and caufes of certain difeafes? And what ufeful confequences, more or lefs grounded on experience, may be thence deduced for improving the practice of medicine?"

remedies;

3. "To what extent has the new chemiftry been of ufe to afford precife notions concerning the action of certain external and internal remedies long in ufe or lately recommended? And what advantages may be obtained from a more exact knowledge in this refpect in the treatment of certain diforders?"

explanations.

As many philofophers have mixed hypothefes of flight foundation with the applications they have made of the principles of the new chemiftry to the functions of the human body, which is no doubt very hurtful to the progrefs of fcience, which might be fo highly improved, if, according to the rule of Lavoifier, nothing were adopted either in chemiftry or the application of its principles, but what fhall be founded on decifive experiments; the Society defires that thofe who fhall anfwer thefe queftions fhould precifely diftinguifh facts from hypothefes; and that with regard to the latter, they fhould be merely pointed out, and their flight foundation briefly fhown. For the principal aim of the Society in thefe queftions, is to procure thofe who exercife the practice of medicine and furgery in the Batavian Republic, and are not fufficiently acquainted with the progrefs of the new chemiftry and the application of its well eftablifhed principles to phyfiology, pathology, and theurapeutics, fuch memoirs as may eafily fhow them what light the new chemiftry has in effect thrown upon thefe fciences; and alfo thofe doctrines which, being of flight foundation, too haftily adopted, or in themfelves doubtful, are not entitled to confidence.

Judgment will be given feparately upon the memoirs refpecting each of thefe queftions. They who propofe to anfwer more than one, are therefore requefted to do it feparately.

The above were propofed in 1799.

Philofophy of fire in the ma-

4. "What are the principles of the philofophy of fire, refpecting the production, the communication, and confinement of

of heat, which are neceſſary to be known in order to form a right judgment of the methods of uſing combuſtibles; and how can we, according to theſe principles, improve the fires for warming apartments and ſtoves for kitchen uſe, in order to ſave as much as poſſible the combuſtibles in uſe in this country?" nagement of combuſtibles, &c.

The above was propoſed in 1801.

5. "What is actually known reſpecting the cauſes of the corruption of ſtagnant water, and is it poſſible to deduce from what is known or may be proved by deciſive experiments, what are the moſt effectual and ſafe means to prevent the corruption of ſtagnant waters?" ſtagnant water.

Prepared in 1801.

The prize for each queſtion is a gold medal of thirty ducats. The memoirs may be written in Dutch, French, or German; but only in Italic characters. They muſt be accompanied with a ſealed billet, containing the name and addreſs of the writer, and ſent to M. Van Marum, Secretary of the Society. Prize, a medal of 30 ducats.

The Batavian Society has propoſed as one of the prized ſubjects to be decided on the 1ſt of May 1802, the following queſtion: "What do we learn from the lateſt diſcoveries in chemiſtry reſpecting the nature of fermentation, and what advantages may reſult with regard to certain trades in which fermentative matters are uſed?" It was not thought fit to adjudge the prize; but without opening a new concurrence, the Society has engaged the author of a memoir, of which the device is, *Tout eſt important dans les voïes de la nature,* to ſupport his theory upon more deciſive experiments, and to extend its application to a greater number of manufactories; with promiſe of the crown if he ſhall accompliſh his purpoſe before the end of 1803 or 1804. Fermentation.

Sixth Deſcent of Garnerin with his Parachute.

On the 21ſt of September 1802, at ſeventeen minutes paſt five in the evening, the aeronaut Garnerin ſent off from the Parade in North Audley Street, an exploring balloon, which took a north-eaſt direction until it attained a very conſiderable elevation, and ſoon diſappeared. At fifty-eight minutes after five the great balloon itſelf aſcended with the parachute depending, and beneath it the cylindrical baſket in which Garnerin himſelf ſtood and repeatedly waved his flag. Its ſlow Account of Garnerin's deſcent by a parachute.

aſcent

afcent in the calmnefs of a beautiful evening, with the fixed and ftill attention of the inhabitants of a great metropolis, to the amount of many hundreds of thoufands, produced a fingular and very impreffive effect on the mind. This paufe of expectation which lafted nine minutes and a quarter, and carried the aeronaut fo high as not to be perfonally diftinguifhed, was fuddenly terminated by the feparation of the parachute from the balloon. It fuddenly defcended for near a fecond, and then expanded. The balloon proceeded immediately to the fouth, and was found the next day in Kent, about twelve miles out of town. Admiration and furprize now engaged the minds of all the fpectators; which foon gave place to a marked fentiment of terror from the extreme vibrations of the apparatus. The defcent appeared flow and regular, but its vibrations (performed in about fix feconds each) were fo violent as frequently to carry the bafket nearly as high as the level of the parachute itfelf, which collapfed at its lower edge every time, fo as to be nearly half clofed, and opened again when it refumed its perpendicular fituation. However firm the confidence of this daring adventurer may have been in the truth of his combinations, he muft have have found himfelf in a fituation requiring the exertions of all his courage. He defcended in fafety in a field near Pancras, having been very nearly fix minutes in his defcent. He feemed to have been upwards of a mile high, or probably about fix thoufand feet; and on this fuppofition he fell at the rate of fixteen feet in a fecond. This is the velocity that would have been produced by jumping from the height of about four feet, and muft therefore have been very fafe.

Velocity of his fall.

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AND
THE ARTS.

NOVEMBER, 1802.

ARTICLE I.

Letter from THOMAS YOUNG, *M. D. F. R. S. In Reply to Mr.* GOUGH's *Letter, at Page 36 of the present Volume. On the Phenomena of Sound.*

To Mr. NICHOLSON.

SIR,

I AM not certain that any anfwer, that I can make to Mr. Gough's reply to my letter, will add very materially to the elucidation of the fubject in difpute; a part of my object is already attained, for Mr. Gough feems to be no longer difpofed to contend for Dr. Smith's infallibility, and he appears to have formed in fome meafure a more complete idea of my original opinions, while he imagines that I have introduced fome new modifications of them. I cannot however avoid remarking, that Mr. Gough has wholly omitted to notice the fundamental fact, which I ftated as affording the moft fatisfactory of all proofs of the coalefcence of founds; that is, the production of a faint, but very audible, graver found from the union of two acuter ones, a phenomenon fo well know to muficians, that I can fcarcely fuppofe a perfon, who is ignorant of it, properly

Further difcuffion apparently unneceffary.

But Mr. Gough has not noticed the grave harmonics.

The facts.

qualified to discuss the subject of harmonics; and so important with regard to the question at present in dispute, that I can scarcely conceive how a person, acquainted with it, can omit to endeavour to reconcile it with his opinions, especially when challenged by his opponent to take it into consideration. Here is a product of combination which possesses properties totally different from those of its constituent parts; its pitch is always much lower, its quality of tone is perfectly singular, and the hearer is sometimes almost inclined to imagine its direction to be different from that of the original sounds; a circumstance which he sometimes expresses by saying, that the sound "rings in his ears." Mr. Gough cannot controvert this fact by any reasoning: if he can persuade me that such a sound is a mixture and not a compound by partial coalescence, we shall then be nearly of the same opinion.

The grave harmonic is not a mixture, but a compound.

Illustration from a compound pendulum.

Will Mr. Gough deny, that when a small pendulum is suspended to the heavy weight of a larger one, its motion is compounded of larger and smaller vibrations? Or will he insist that the result is a mixed motion, and not a compound one? The eye certainly follows each vibration separately, although the vibrations may be so proportioned that a joint order and sequence may be observed, which may produce effects peculiar to the combination, and discoverable in neither of the original motions. The comparison appears to me to be perfectly apposite, and if Mr. Gough allows it, I can scarcely imagine what cause there is left for further controversy.

I beg leave once more to recommend the grave harmonics to Mr. Gough's notice, and to intreat him to suspend any further reply to my remarks until he has made himself thoroughly acquainted with these interesting phenomena.

I am, Sir,

Your faithful humble servant,

THOMAS YOUNG.

Royal Institution,
Oct. 12, 1802.

II. *On*

II.

On the Galvanic Effect of very minute Particles of Zinc and Copper in Water. In a Letter from Mr. WILLIAM WILSON.

To Mr. NICHOLSON.

SIR, *London, October* 11, 1802.

I WAS lately grinding fome fmall plates of copper upon fome plates of zinc (with fine emery), and after having wafhed them in a cup of water to cleanfe them, I left them about five hours. When I returned I found the water in the cup covered with a dirty froth, and upon taking up the cup to examine it, the agitation caufed innumerable bubbles of air to rife from the fediment at the bottom of the water. By continuing to agitate the water for feveral minutes, they decreafed in quantity, and at laft difappeared. I now left it about four or five hours, and then examined it again, when the bubbles were nearly as copious as ever, and, with nearly the fame quantity of agitation, again ceafed to be produced.

Copper and zinc were ground with emery. The mud after fubfidence emitted gas; and at remote intervals.

I examined this water every four or five hours for five days, and the appearance was the fame, though weaker and weaker. After the fifth day I could not find that any bubbles were generated; I therefore (fuppofing this appearance to be occafioned by the particles of copper and zinc forming a kind of galvanic feries) took equal quantities of filings of copper and zinc and put them into a glafs of water, and ftirred it about to mix them as much as poffible. In about three hours afterwards there were fome bubbles of air adhering to the filings, which by agitation rofe to the top of the water; and the fame phenomenon was repeatedly exhibited. In fhort thefe metallic particles acted precifely fimilar to the former, with only this difference: It required more violent agitation; the air bubbles were larger, and the property difappeared much fooner, for there were no bubbles generated after the third day. This difference I fuppofed to be owing to the difference in the fize of the particles of the metals; for in the firft cafe the particles, by reafon of their fmallnefs, would form a more numerous feries and extenfive furface of contact, than the filings could in the latter.

The effect lafted five days.

Copper and zinc filings were tried, with the fame effect.

Explanation of the difference.

The impalpable powder of the metals.

Laſtly, I repeated the firſt experiment by waſhing ſome emery very fine, and ſlightly ground with it a plate of zinc on another of copper, and then waſhed them in a glaſs of clear water. It was ſeveral hours before the ſediment had all fallen to the bottom of the glaſs; but when it had, it acted nearly with the ſame power as in the firſt caſe, and continued to act quite as long *.

If, Sir, you ſhould judge the above account worthy of a place in your Philoſophical Journal, I ſhall conſider myſelf very much obliged by its inſertion.

I am, Sir,

Your obedient humble ſervant,

WM. WILSON.

III.

On the Colours obtained from Metallic Oxides, and fixed by Means of Fuſion on different Vitreous Bodies. By ALEX. BRONGNIART, *Director of the National Manufactory of Porcelain at Sévres, Engineer of Mines, &c.*

(Concluded from Page 101.)

Concerning the Red, Roſe, and Brown Colours obtained from Iron.

Red colours from the oxide of iron.

THESE colours are made from red oxidated iron prepared with nitric acid. The oxides are calcined ſtill more by expoſing them to the action of fire. If too much heated, they change to a brown.

Compoſition.

Their flux is compoſed of borax, ſand, and minium in ſmall quantity.

They may be ſubſtituted for the oxide of gold.

Theſe are the oxides which afford the roſe and red colours that may be ſubſtituted inſtead of the ſame colours made from oxide of gold. If properly applied on hard porcelain, they never change. I have made roſes with theſe colours, and there

* Theſe galvanic facts appear to conſtitute one of the caſes detailed with larger pieces of metal, and obſcurely explained by Fabbroni in his curious paper in the Philoſ. Journal, quarto ſeries, IV. 120. —W. N.

was

was no difference between the flower before and after the baking, except that brilliancy which colours naturally receive by fusion.

The colours may either be previously fused or not at pleasure.

In a violent fire they either partly disappear, or produce a dull and brick-dust red colour, which is not at all agreeable. How affected by violent heat.

Their composition is the same either for tender porcelain or for glass. They do not change on the latter, but on the former they almost intirely disappear by the first fire, and they must be laid on very heavily in order to have any part visible. They are good for glass and tender porcelain;

It is to the presence of lead in their glaze that this singular effect must be attributed. I have ascertained this by a very simple experiment. I placed this colour on window-glass and fired it very strongly, and it did not change. but must be heavily laid on the latter.

I then covered some parts of it with minium, and again exposed it to the fire. The colour totally disappeared in those places where the red oxide of lead had been applied. Experiments;

When I performed this operation on a larger scale in closed vessels, a large quantity of oxigen gas was disengaged.

This observation I think clearly proves the effect of oxided lead as a discolourer of glass: we see that it does not operate, as has been supposed, by burning combustible impurities in the glass, but by dissolving, discolouring, or volatilizing the oxide of iron which may affect its clearness. which shew that oxide of lead *discolours* glass.

Concerning the Yellows.

Yellows are colours that require much precaution in fabricating, on account of the lead they contain; which sometimes, by approaching to the metallic state, produces black spots. Yellows are formed by the oxides of lead and antimony;

The yellows of hard and tender porcelain are the same. They are composed of oxide of lead, white oxide of antimony, and sand.

Oxide of tin is sometimes added; and when it is required very lively and resembling the colour of the marigold, red oxide of iron is added, the very deep colour of which disappears during the previous fusion they undergo, on account of the lead contained in this yellow. When these colours are once made, they do not change; they disappear almost intirely in the porcelain fire. and sometimes tin. They are not changeable.

These

Not applicable to glafs.

These yellows cannot be applied to glafs, they are opaque and muddy. That employed by the ancient painters on glafs is, on the contrary, beautifully tranfparent, very brilliant, and of a colour approaching to gold. The proceffes they give indicate that it contained a mixture of filver; but when exactly followed, they afford nothing fatisfactory. Citizen Meraud, whom I have before quoted, has fucceeded in making it as beautiful as that of the ancient painters on glafs, by employing muriate of filver, oxide of zinc, white clay, and the yellow oxide of iron. Thefe colours are applied to the glafs fimply ground, and without flux. The oxide of iron gives the yellow nearly the fame tinge as it ought to have after the baking, and contributes, with the clay and oxide of zinc, to decompofe the muriate of filver without difoxidating the filver itfelf. A powder remains after baking which does not penetrate the glafs, and may be eafily cleared off.

A yellow for glafs equal in beauty to the ancient.

This yellow, when employed in greater quantity, affords deeper fhades, and produces a reddifh yellow.

Concerning the Blues.

Blues from cobalt require nothing but care.

Thefe are known to be obtained from the oxide of cobalt. Their preparation is known to every chemift. The fuperiority at Sévres, fo juftly reputed for the beauty of its blues, is owing merely to the care taken in its fabrication, and to the quality of the porcelain, which appears more proper to receive it on account of the violent fire it can fupport.

Oxide of cobalt is volatile.

I have obferved one fact refpecting the oxide of cobalt, which is perhaps not known to chemifts: It is volatile in a violent heat; to this property muft be attributed the bluifh tint which the white (bordering upon the blue) always receives. I purpofely put into the fame cafe a white piece next to a blue; the fide of the white piece that was turned towards the blue, became very bluifh.

Blue grounds.

The blue of hard porcelain, prepared for what is called a blue ground by ftrong fire, is fufed with feld-fpar: the folvent for tender porcelain is filex, potafh, and lead; it is not volatilized like the preceding, becaufe the fire is much inferior to that of the hard porcelain.

Thefe colours being previoufly fufed, do not in the leaft change when applied.

Blue for glafs.

The blues on glafs are the fame as for tender porcelain.

Concerning

Concerning Greens.

The greens employed in painting are made with the green oxide of copper, or ſometimes with a mixture of yellow and blue. They muſt be previouſly melted with their flux; without this precaution they would become black; but they do not change after the firſt fuſion. **Greens; from copper,**

They muſt not be treated with a violent fire, or they would totally diſappear. The green grounds by ſtrong heat are made with the oxides of cobalt and nickel, but it is only a browniſh green. **not very fixed.**

The bluiſh greens named ſky-blue, formerly a colour very much in eſteem, can only be uſed on tender porcelain; they always ſcale off from hard porcelain, becauſe there is potaſh in their compoſition. **Blue-green.**

Theſe greens cannot be uſed on glaſs, becauſe they afford a dirty colour: It is neceſſary to put a yellow on one ſide and a more or leſs pale blue on the other, in order to produce a green. This colour may likewiſe be fabricated by mixing a blue with the yellow oxide of iron. I hope to obtain a green from the oxide of chrome; and the experiments I have made promiſe to be attended with ſucceſs. The pure chromate of lead, fixed on porcelain by means of a ſtrong fire, has already afforded me a very deep and very fixed blue of conſiderable beauty. **Theſe greens do not work well on glaſs.** **Blues for glaſs.**

Concerning Biſtres and Brown Reds.

Theſe are obtained by mixtures of different proportions of manganeſe, brown oxide of copper, and the oxide of iron called umber. They are likewiſe previouſly fuſed with their ſolvents, ſo that they do not in the leaſt change on tender porcelain; lead not having the ſame action on the oxide of manganeſe as it has on that of iron. I am convinced of this by an experiment ſimilar to that I have already related. **Brown reds from ſeveral oxides mixed,**

This colour may be employed very well on glaſs. **anſwer on glaſs.**

The brown red grounds by ſtrong heat, known by the name of *fonds caille,* are made in the ſame manner. Feld-ſpar is their flux. There is no titanium in their compoſition, though generally aſſerted in books. Titanium was not known at Sévres when I firſt came to that manufactory. I have treated this ſingular metal in various ways, and I never obtained any grounds but a ſlight obſcure yellow, and very uncertain in its quality. **Grounds.**

Concerning

Concerning the Blacks.

Blacks. Black colours are the moſt difficult to be obtained very beautiful. There is no metallic oxide that ſingly affords a fine black. Manganeſe gives the beſt. Iron an opaque, dull, bliſtered black, which eaſily turns to red; the makers of colours have therefore combined ſeveral metallic oxides which ſingly do not afford blacks, and they have obtained a very beautiful colour, but it is ſubject to ſcale and become dull.

Oxides for composing black. Theſe oxides are thoſe of manganeſe, the brown oxides of copper, and a little of that of cobalt. Grey is obtained by ſuppreſſing the copper and increaſing the quantity of flux.

Fine black at Sévres, The Sévres manufactory is the only one which has as yet produced beautiful blacks with a ſtrong fire. This is more owing to the quality of the biſcuit than to any peculiarity of proceſs. It is by a mixture of blue with the oxides of manganeſe and iron, that they make this very brilliant black.

for glaſs. The blacks for opaque glaſſes are made the ſame as for painting, by giving different doſes of ſolvent.

Mixtures of colour. I have ſhewn the principles of fabricating each principal colour: it is clear that by mixing theſe colours together, all poſſible ſhades may be obtained: and alſo, that care in the preparation, choice of materials, and juſt proportions of doſes, muſt exhibit very ſenſible differences to the experienced eye of a painter. A knowledge of the compoſition of colours does not give the requiſite care and neatneſs in making them up.

Recapitulation. On recapitulating the facts I have juſt ſtated, in order to preſent them in a general view, we ſee, *firſt*, that amongſt the colours uſually employed for hard porcelain, one only is ſuſceptible of change; namely, the carmine; and this may be replaced by the reds of iron, and then no colour changes.

Unchangeable ſpecimens. I have preſented to the Inſtitute an unbaked head made in this manner, and a painting of two roſes, the one baked, the other in its firſt ſtate. There was not any difference between them.

Secondly, That amongſt the colours of ſoft porcelain and enamel ſeveral change conſiderably, particularly the reds of iron and gold with the yellows, greens, and browns. None have been ſubſtituted inſtead of them, this ſpecies of painting being almoſt abandoned.

Thirdly,

Thirdly, That ſeveral of theſe colours change likewiſe upon the glaſs by becoming perfectly tranſparent, particularly the yellows and violets.

Fourthly, That neither an additional calcination, nor a previous fuſion, as has been ſuſpected, will prevent them from changing. For this method alters the colours that change, and does nothing to the reſt. The change which ſeveral colours undergo on tender porcelain and on glaſs, does not therefore relate to the nature of their compoſition, but rather to that of the body on which they are placed.

Conſequently by ſuppreſſing the carmine of gold from the colours of hard porcelain, we ſhall have a ſeries of unchangeable colours, which would be abſolutely ſimilar to thoſe preſented to the Inſtitute in the year 6 *.

IV.

Experiments and Obſervations on certain ſtony and metalline Subſtances, which at different Times are ſaid to have fallen on the Earth; alſo on various Kinds of native Iron. By Edward Howard, *Eſq. F. R. S. From the Philoſophical Tranſactions,* 1802.

(Concluded from Page 101.*)*

Deſcription of various Kinds of native Iron. By the Count de Bournon.

Remarks on the manner in which native iron may have been formed.

THE great number of particles of iron, in a perfectly metallic ſtate, contained in the ſtone from Bohemia, and the ſaid particles being ſo near each other, naturally lead to ſome reflections reſpecting the exiſtence of native iron, which, by many mineralogiſts, is ſtill conſidered as problematical. Let us ſuppoſe for a moment, that theſe particles of iron were to approach

* The preparation of colours for lively grounds by ſtrong fire is under the care of Cit. Chanon, and that of the colours for painting to Cit. Meraud already quoted. It is to their care and intelligence in this chemical art, that the manufacture of Sévres is indebted for the preſervation of its beautiful colours, which they have very much varied and improved.---B.

approach ftill more nearly to each other, fo as abfolutely to come into contact, and in that manner to form a kind of chain, folded upon itfelf in the interior part of the fubftance, and leaving a great number of cavities between the links of the chain fo folded. Let us then fuppofe, that the earthy fubftance with which thefe cavities are filled, being very porous, and having but a fmall degree of confiftence, fhould (as may happen by a variety of caufes) be deftroyed. It is plain, that if fuch a deftruction were to take place, the iron alone would remain; and, being thus left bare, it would appear in the form of a mafs, more or lefs confiderable, of a cellular texture, and as it were ramified; fuch a form, in fhort, as that in which moft of the native irons we are acquainted with have been found. May it not be fair to attribute to fuch an origin, the native iron found in Bohemia, a fpecimen of which was prefented by the Academy of Freyberg to Baron Born, and which came, with the reft of his collection, into the hands of Mr. Greville? May not fuch alfo, notwithftanding the enormity of its bulk, be the origin of the mafs of native iron found in Siberia, near Mount Kemirs, by the celebrated Pallas?

The ftones examined by Mr. Howard contained nickel,

We have already feen, in the refults of the analyfes made by Mr. Howard, of the various ftones above defcribed, that he conftantly found a certain proportion of nickel mixed with the iron they contained. This circumftance recals to our notice the obfervations that were made by Mr. Prouft, fome time ago, refpecting the mixture of nickel in the native iron of South America; and tends to give fome additional fupport to the opinion hinted at in the foregoing paragraph.

and fo do the S. American iron.

The circumftances juft mentioned, naturally gave to Mr. Howard, as well as to me, a defire to know whether the native iron from Siberia, and that from Bohemia, were alfo mixed with nickel. Mr. Howard, confequently, loft no time in proceeding upon this important inveftigation. The native iron of Siberia prefents fome very interefting peculiarities, and has often been referred to, but has not yet been properly defcribed; it is therefore with great pleafure that I add the following defcription of it, and of fome other kinds of native iron, to the defcription I have already given of the various ftones faid to have fallen on the earth.

The Siberian native iron;

I feel the greater fatisfaction in doing this, as the noble collection of Mr. Greville contains two fpecimens of this iron, in

perfect

perfect condition; one of which weighs several pounds, and was sent to Mr. Greville by Mr. Pallas himself: on this account, therefore, I enjoy an advantage that many of the authors who have spoken of this iron probably wanted.

One of these pieces has a cellular and ramified texture, analogous to that of some very porous and light volcanic scoria: this is the usual texture of the specimens of this kind of iron, which are preserved in the various mineralogical collections in Europe. When it is attentively examined, there may be perceived in it, not only empty cells, but also impressions or cavities, of greater or less depth, and sometimes perfectly round, which appear evidently to be the result of the compression of hard bodies. which were situated there, and which, when they came away, left the surface of these cavities quite smooth, and having the lustre of polished metal. Here and there, in some of these cavities, there remains a transparent substance, of a yellowish green colour, of which I shall treat more particularly, when I come to the description of the second of the specimens above mentioned. It is very clear, that the cavities here spoken of owe their existence to this transparent substance; and that the polish of the cavities arises merely from the compression of the said substance. and is the natural consequence of its surface having been in perfect contact with that of the iron. described, cellular, and mixed with a yellowish green transparent matter.

This iron is very malleable: it may be easily cut with a knife; and may be as easily flattened or extended by means of a hammer. Its specific gravity is 6487; which, however, is very much under that of iron which has been merely melted, and has not been forged. The specific gravity of the native iron of Bohemia, which is nearly as malleable and as easy to be cut, is still less: I found it not to exceed 6146. This low degree of gravity, appears to be owing partly to the oxidizement of the surface of the iron, and partly to there being, in the interior part of its substance, a number of small cavities, which are often rendered visible by fracture, and which have their surfaces also oxidized. The fracture of this iron, presents the same shining and silvery white colour as the common cast iron, known by the name of white cast iron; but its grain is much smoother and finer: it is also much more malleable when cold. Bergman says that this iron is brittle, when heated to a red heat. I have frequently tried it in that state, and This iron very malleable. Its fracture is white, but it is not red-short.

and have conſtantly found it to be malleable. The ſame remark may be applied to the native iron from South America; and alſo to that from Senegal.

Another ſpecimen; more compact,

The ſecond of the two ſpecimens mentioned above, and which weighs ſeveral pounds, preſents an aſpect that differs, in ſome reſpects, from that of the preceding ſpecimen. The moſt conſiderable part of it forms a ſolid compact maſs, in which there is not to be perceived the ſmalleſt appearance of pores or cavities; but there ariſes upon its ſurface, a kind of ramified or cellular part, ſimilar, in every reſpect, to the ſpecimen already deſcribed, and every where completely connected with the ſubſtance of the maſs itſelf.

conſiſting of iron and the greeniſh tranſparent matter, which fills its cavities.

If the compact part of this piece is examined with attention, it will be perceived, that it is not entirely compoſed of iron in the metallic ſtate, but that it is mixed with nearly an equal quantity of the tranſparent ſubſtance of a yellowiſh green colour, (ſometimes alſo of a greeniſh yellow,) already ſpoken of in the deſcription of the other ſpecimen. This ſubſtance is mixed with the iron, in ſuch a manner, that if the whole of the former could be removed, the remaining part would conſiſt merely of iron in the metallic ſtate, and would preſent the ſame cellular appearance as the preceding ſpecimen, and the ramified or cellular part of the ſpecimen now deſcribed.

The green ſtony part looks like glaſs:

This ſtony part, ſeparated from the iron, appears in the form of ſmall nodules, generally of an irregular ſhape, but ſometimes nearly globular: they have a perfectly ſmooth and ſhining ſurface, ſo as very often to preſent the appearance of ſmall balls of glaſs; a circumſtance that has led many perſons to ſuppoſe them the reſult of a real vitrification. Some of theſe nodules have ſeveral irregular facets, produced by the compreſſion of the iron in which they were incloſed; but I have never obſerved in them, any appearances that could lead me to ſuſpect they had the ſlighteſt tendency whatever to aſſume a determined cryſtalline form.

is always tranſparent; cuts glaſs, but not quartz; fracture conchoidal; electric by friction;

This ſubſtance is always more or leſs tranſparent. It is ſufficiently hard to cut glaſs; but has no effect upon quartz. It is very brittle: its fracture is uſually conchoid; but I could not perceive that it broke in any particular direction, in ſuch a way that I could conſider the fracture as a natural one. It becomes electric by friction. Its ſpecific gravity is from 3263 to 3300. It is very refractory: I kept it, for ſome time, expoſed

posed to a degree of heat sufficiently strong to oxidize, to a considerable depth, the iron crucible in which it was placed, without its having undergone any alteration, except that of having acquired a greater degree of intensity in its colour. Its transparency was not at all diminished. I think, therefore, there is not the smallest reason to allow any probability to the opinion that it ought to be considered as a kind of glass.

not fusible in an iron vessel.

Of all substances hitherto known, that with which it seems to have the greatest analogy, is the peridot, (the chrysolite of Werner,) to which some mineralogists have referred it. The result of Mr. Howard's analysis of it, is nearly the same as that of the analysis of the peridot, made by Mr. Klaproth.

It resembles the peridot;

The hardness and infusibility of this substance are nearly the same as those of the peridot; but it seems to have a rather less degree of specific gravity: that of two very perfect crystals of peridot, I found to be from 3340 to 3375. The crystalline forms of the substance here described, if ever we should be able to determine them, would clear up our doubts respecting the analogy between the two substances. If we consider the compact part of the specimen now treated of, particularly the strong connection that appears to exist between the iron and the transparent substance, and the great resistance we experience when we attempt to separate them, we cannot help being surprised, that almost all the specimens of this mass of metallic iron that have been brought to Europe, are in the cellular state already described, owing apparently to the total, or almost total, destruction of the transparent substance. But, besides the fragility of this substance, the specimen in question helps very much to explain the above cirumstance, inasmuch as many of the nodules of the transparent substance belonging to it, are in a state of real decomposition. In that state, they are changed into a white opaque substance, which, upon being lightly pressed or squeezed between the fingers, crumbles into a gritty dry powder. This decomposition may be observed to have taken place in various degrees: in many of the nodules, the substance is merely become friable, without being much altered in its appearance; whereas, some of those which are in a state of complete decomposition, are of an ochreous reddish yellow colour; it is, however, easy to distinguish that this colour does not belong to them, but is owing only to the oxiidizement of the adjacent particles of iron.

in hardness and infusibility.

It is curious that it should be destructible by the weather;

but it undoubtedly is so.

From

This fact explains the ſtructure of native iron, &c.

From the above obſervations, it will not be difficult to conceive the poſſibility of the total, or nearly total, deſtruction of the tranſparent ſubſtance; and alſo, the appearance the pieces of iron muſt naturally preſent, when deprived of it. I cannot help obſerving likewiſe, that there appears to exiſt a very intereſting analogy, between theſe tranſparent nodules and the globules I deſcribed as making part of the ſtones ſaid to have fallen on the earth. This analogy, though not a very ſtrong one, may lead us to ſuppoſe that the two ſubſtances are ſimilar in their nature, but that the globules are leſs pure, and contain a greater quantity of iron.

The Bohemian iron in ſome meaſure reſembles the Siberian.

The native iron from Bohemia is a compact maſs, ſimilar to the compact part of the large ſpecimen of iron from Siberia, which has juſt been deſcribed: like that, alſo, it contains a number of globular bodies or nodules; but they are not in ſuch great proportion as in the Siberian iron. They are beſides perfectly opaque, and very much reſemble the moſt compact of the globules belonging to the ſtones ſaid to have fallen on the earth.

EXAMINATION OF THE IRON FROM SOUTH AMERICA.

S. American iron contains 10 per cent. of nickel.

I have already obſerved, that my experiments coincided with thoſe of Mr. Prouſt. He obtained 50 grains of ſulphate of nickel, from 100 of this maſs. The proceſs I have ſo frequently mentioned, yielded me 80 grains of oxide of iron from 62 of the metal; which indicates about 7½ of nickel, or about 10 per cent.

EXAMINATION OF THE SIBERIAN IRON.

Siberian iron 17 per cent. &c.

100 grains of this iron, gave 127 of oxide of iron: hence, it ſhould contain about 17 per cent. of nickel.

The yellow matter.

The yellow ſubſtance belonging to this iron, was analyzed in the ſame way as the globular bodies, and the earthy parts, of the ſtone from Benares.

The proportions, reſulting from the analyſis of 50 grains, and from ſome previous experiments on other particles, were,

Silica

Silica - - - -	27
Magnesia - - - -	$13\frac{1}{2}$
Oxide of iron - - - -	$8\frac{1}{2}$
Oxide of nickel - - -	$\frac{1}{2}$
	$49\frac{1}{2}$

EXAMINATION OF THE BOHEMIAN IRON.

$26\frac{1}{2}$ grains of this metal, left about $1\frac{1}{2}$ grain of earthy matter, insoluble in nitric acid; and, by ammonia, afforded 30 grains of oxide of iron, inducing an estimation of nearly 5 of nickel. Bohemian iron 17 per cent. do.

EXAMINATION OF IRON FROM SENEGAL, BROUGHT BY GENERAL O'HARA, AND GIVEN TO ME BY MR. HATCHETT.

In this experiment, 199 grains of oxide were produced from 145 grains of metal; hence, there may be an estimation of 8 grains in 145, or between 5 and 6 per cent. of nickel. Senegal iron 5 or 6 per cent. ditto.

It will appear, from a collected view of the preceding pages and authorities, that a number of stones asserted to have fallen under similar circumstances, have precisely the same characters. The stones from Benares, the stone from Yorkshire, that from Sienna, and a fragment of one from Bohemia, have a relation to each other not to be questioned. Summary of the characters of the stones fallen on the earth.

1st. They have all pyrites of a peculiar character.

2dly. They have all a coating of black oxide of iron.

3dly. They all contain an alloy of iron and nickel. And,

4thly. The earths which serve to them as a sort of connecting medium, correspond in their nature, and nearly in their proportions. Pyrites, oxide of iron, nickel, earths correspondent.

Moreover, in the stones from Benares, pyrites and globular bodies are exceedingly distinct. In the others they are more or less definite; and that from Sienna had one of its globules transparent. Meteors, or lightning, attended the descent of the stones at Benares, and at Sienna. Such coincidence of circumstances, and the unquestionable authorities I have adduced, must, I imagine, remove all doubt as to the descent of these stony substances; for, to disbelieve on the mere ground of incomprehensibility, would be to dispute most of the works of nature. Meteors or lightning accompanying them.

Native irons.

Respecting the kinds of iron called native, they all contain nickel. The mass in South America is hollow, has concavities, and appears to have been in a soft or welding state, because it has received various impressions.

The Siberian iron has globular concavities, in part filled with a transparent substance, which, the proportional quantity of oxide of iron excepted, has nearly the composition of the globules in the stone from Benares.

The iron from Bohemia adheres to earthy matter studded with globular bodies.

The Senegal iron had been completely mutilated before it came under my examination.

From these facts, I shall draw no conclusion, but submit the following queries.

Have these all the same origin? and from meteors?

1st. Have not all fallen stones, and what are called native irons, the same origin?

2dly. Are all, or any, the produce or the bodies of meteors?

And, lastly, Might not the stone from Yorkshire have formed a meteor in regions too elevated to be discovered?

Specimens of the Benares and Yorkshire stones have been deposited, by the President, in the British Museum.

V.

*Experiments and Observations on the Heat and Cold produced by the Mechanical Condensation and Rarefaction of Air *. By* JOHN DALTON.

Well known facts that the thermometer rises in condensed and falls in rarefied air.

If a thermometer be inclosed in a receiver and the air suddenly condensed, the thermometer rises a few degrees above the temperature of the atmosphere; and if the air be exhausted from a receiver inclosing a thermometer, the mercury sinks a few degrees immediately; but in both cases after some time it resumes its former station. These facts are well known to philosophers of the present age, but they do not all agree in the explanation of them. Thinking the subject worthy of elucidation, I was induced to institute a series of experiments for the purpose, which I apprehend have led to a clear demonstration of the cause of the phenomena, and moreover makes

* Manchester Memoirs, V, 515.

the facts themſelves appear in a ſomewhat different point of view from what they are ſeen in at the firſt moment.

One circumſtance is very remarkable, that whether the mercury riſes or falls in theſe inſtances, it is done *very rapidly*; whereas in the open air, if a thermometer be only two or three degrees above or below the temperature, it moves very ſlowly. This ſeems to have ſuggeſted to every one the idea, that the elaſticity of the glaſs bulb of the thermometer has a principal ſhare in producing the effect, by cauſing the bulb to yield a little to the preſſure of the air. It has however been found upon trial that the ſame effects take place whether the thermometer is ſealed or not. My experiments accord with this, having made a thermometer and left it unſealed for the expreſs purpoſe; in all the experiments with condenſed and rarefied air, there was no ſenſible difference obſerved to ariſe from the inequality of preſſure on the external and internal ſurfaces of the bulbs, the ſealed and open thermometers varying the ſame in kind and alſo in degree, except from circumſtances to be noticed hereafter.

This riſe and fall are *very rapid*, whence it was ſuppoſed to ariſe from preſſure on the glaſs bulb; but it happens whether the tube be ſealed or not.

It being certain then that a real change of temperature takes place, it remained to determine the quantity and manner of that change. Having choſen a ſmall and conſequently ſenſible thermometer, with a ſcale of degrees ſufficiently large to admit of diſtinguiſhing one tenth of a degree, I proceeded to aſcertain ſeveral facts experimentally.

Conſeq. a real change of temperature takes place.

EXPERIMENT 1.

Took a receiver, the capacity of which was about 120 cubic inches, and ſuſpended the thermometer with its clear bulb in the central part of it; then letting the whole acquire the temperature of the room, which was without a fire, I exhauſted the air and afterwards reſtored it, marking the effects upon the thermometer. The medium of ſeveral trials nearly agreeing with each other was as under:

Exp. I. A very ſmall thermometer in a ſmall receiver fell by exhauſtion, and the contrary *very ſuddenly*.

The thermometer in the air of the room ſtood at -	36°.8
——————— ſunk upon exhauſtion to - - -	34 .7
——————— roſe when the air was reſtored to	38 .9

The *ſuddenneſs* of the fall and riſe puzzled me moſt: after reflecting upon it for ſome time, I conjectured that the real change of temperature of the air or medium was much greater than

whence it was inferred, that the real change of temperature was very great; but not very effective because so momentary.

than the thermometer indicated, but that the inequality existed only for a few seconds of time, because the receiver, &c. immediately impart heat to or abstract it from so small a quantity of air as 120 cubic inches, which are only equal to 40 grains in weight. The phenomena of the thermometer seemed very well to accord with the supposition of *great heat or cold* acting upon it for a few seconds only.

EXPERIMENT 2.

Exp. 2. A large and a small thermometer were cooled differently.

Pursuing this idea, I imagined that if two thermometers whose bulbs were very unequal in magnitude were inclosed together, the smaller bulb ought to give the greater variation: accordingly I inclosed two, the diameters of their bulbs being .35 and .65 of an inch respectively; and having exhausted the air and restored it again repeatedly in succession, and found a mean of the variations, that of the small bulb was 2°.8, and that of the large, 2°.2.

EXPERIMENT 3.

Exp. 3. And the same therm. was more cooled the more remote from conductors of heat.

Repeated the exhaustion with the small thermometer inclosed in three different circumstances successively; 1st with the bulb in the centre of the receiver; 2d with the bulb resting on the wet leather of the plate; and 3d with the bulb resting against the side of the receiver.

1st Case—Reduced by exhaustion - -	2°.45
2d Case - - - - - -	2 .15
3d Case - - - - - - -	1 .2
1st Case—Raised by restoring the air - -	4 .05
2d Case - - - - -	2 .25
3d Case - - - - - -	2 .8

EXPERIMENT 4.

Exp. 4. A cubic inch of water was sensibly cooled.

Inclosed a wine glass with about a cubic inch of water in it, containing the bulb of a thermometer, in a receiver; and, exhausting the air, the thermometer sunk half a degree suddenly, and then continued stationary: upon restoring the air it suddenly rose half a degree.

From all which it follows that the real change was much

All these experiments confirmed my conjecture of a much greater degree of heat and cold being produced in these cases than the thermometer points out, but that its continuance is so

ſo ſhort as not to effect a material change in the temperature of the mercury. The following experiments were made to aſcertain what may be the *real* degree of heat and cold generated in thoſe operations.

greater than ſhewn by the thermometer.

EXPERIMENT 5.

The ſame receiver and ſmall thermometer as above being uſed, I found the exhauſtion was effected by working the pump *one minute*. The thermometer ſunk nearly 2° in the firſt half minute, and the remainder, a few tenths of a degree, in the latter half minute. The operation being ſtopped, and things remaining in the ſame ſtate, it required ſome minutes of time before the thermometer recovered *one* degree of the heat loſt. Upon opening the cock, the receiver filled with air in five ſeconds, and the greateſt velocity of the riſing mercury was about the end of that time. The riſing continued for 30 or 40 ſeconds from its commencement, but $\frac{3}{4}$ of the effect were produced in the firſt 10 ſeconds. The greateſt velocity of the riſing mercury is 1° in $3\frac{1}{2}$ ſeconds. After the thermometer had attained its utmoſt height, it began to fall again at the rate of $\frac{1}{10}$ of a degree in a minute.

Exp. 5. The time of ſubſidence much leſs than of ſubſequent riſe in the vacuum; and the time of riſe by the introduction of the external air much leſs than of the ſubſequent fall.

EXPERIMENT 6.

Took the ſame thermometer and heated it to 50° above the temperature of the air, then let it be cooled by the medium of air, and it began to fall at the rate of 1° in $3\frac{1}{2}$ ſeconds.

The two laſt experiments ſeem to prove that when air is let in to the receiver in the ordinary way, *an increaſe of temperature of 50° is produced in the medium within the receiver for* $3\frac{1}{2}$ *ſeconds*. This high temperature is reduced *in a few ſeconds* by the receiver and ſurrounding bodies, to their own temperature.

Exp. 6. By heating the thermometer and letting it cool in the air, it was found that an elevation of 50° was required to produce as rapid an effect as in the former experiment. Such therefore was the real riſe.

EXPERIMENT 7.

On condenſed Air.

Took a large ſpherical glaſs receiver, the capacity of which was ſomething more than twice that of the former (above one gallon), and ſuſpended a thermometer in the centre of it, of a larger bulb than that before uſed; the receiver had a braſs cap and ſtop-cock adapted to it: then doubled the denſity of the air within it by a condenſer. The thermometer roſe 2° or more. Let out the air ſuddenly and the thermometer imme-

Exp. 7. Thermometer in condenſed air roſe ſuddenly when the air was let in.

 diately

diately ſunk each time from 3° to 3°.5; at the ſame time an exceedingly denſe miſt was produced in the receiver, which ſoon ſubſided.

Suſpecting that *aqueous vapour*, which always exiſts in the atmoſphere, and is liable to aſſume the liquid or aërial form according to circumſtances, might be the principal agent in the production of heat and cold by condenſation and rarefaction, I thought that an increaſe of it might produce a greater effect, and that cold air, which contains leſs vapour, might have a leſs effect. The reverſe however was the fact, as appears by the following,

EXPERIMENTS 8 and 9.

Exp. 8 and 9. Theſe effects did not ariſe from the water ſuſpended in the air.

In a cold morning laſt winter when the air was clear and the thermometer without ſtood at 20°, I took the receiver and condenſer into the open air, and let them ſtand for 15 minutes to acquire its temperature; then repeatedly condenſed the air to a double denſity, and ſuddenly liberated it again. On a medium of five trials the mercury fell 3°.3 on opening the cock. The vapour precipitated was whiter than uſual and not nearly ſo denſe.

For the change was leſs when the vapours were moſt abundant.

Again, took the receiver and condenſer into a dyer's ſtove where the temperature was about 100°, and the air abounded with vapour in a tranſparent ſtate: after ſome time, condenſed the air and liberated it as before, when on a medium of five trials the mercury ſunk only 3°, and a very copious miſt was precipitated, ſo denſe that one could but juſt diſtinguiſh the degree of the thermometer through it.

This might be inferred from the known theory.

Theſe experiments ſhew that the greater the quantity of vapour condenſed the leſs is the change of temperature; and that conſequently, if air was entirely free from vapour, the change of temperature would be a *maximum*. Indeed this is clearly conſiſtent with the known law, that when vapour is condenſed, heat is given out. Any proceſs to cool the air muſt be retarded by the condenſation of part of the vapour it contains. Suppoſe for inſtance that a portion of the atmoſphere contained $\frac{1}{60}$ of its weight of aqueous vapour, and that $\frac{3}{5}$ of this vapour were condenſed by 50° of cold; that is, $\frac{1}{100}$ of the whole elaſtic maſs was converted into water; then the heat given out would be ſufficient to raiſe the temperature of the remaining maſs of air and vapour 6 or 8°, which ſufficiently

ciently accounts for the ſmall difference obſerved in the reſults upon warm vapoury air and cold dry air. Hence vapour, far from producing the change of temperature in queſtion, tends to diminiſh the effect.

If any doubt remained with me reſpecting the *real* change of temperature that takes place in the operations related above, it was completely removed by the reſults of the two following experiments.

EXPERIMENT 10.

Incloſed a ſmall graduated glaſs tube of $\frac{1}{15}$ of an inch internal diameter, and 10 inches long, with a ſhort column of mercury in it, in the large receiver; the tube was ſealed at one end and open at the other, ſo that a portion of given capacity was confined by the mercurial column, which was near the open end of the tube, and ſubject to riſe or fall by any variation of elaſticity of the air on either ſide, being a proper manometer: then doubled the denſity of the air in the receiver, and opening the ſtop-cock, the mercurial column ſoon ran up to its former ſtation, but inſtantly turning the cock again, the mercurial column returned or fell down gradually for 5 or 10 ſeconds, to the amount of nearly $\frac{1}{10}$ of the whole aërial column, and then became ſtationary. Again opening the cock, a quantity of air ruſhed out, and the mercury reſumed its original ſtation. Theſe effects were always the ſame, on a repetition of the experiment.

A ſmall manometer was incloſed; the denſity of the ſurrounding air was doubled; then reſtored by a momentary communication from without. The manometer was affected as if the air in the receiver had gradually abſorbed heat and become more elaſtic.

EXPERIMENT 11.

Let the mercurial column of the manometer down by a wire to $\frac{1}{4}$ of the length of the tube from the ſealed end; then exhauſted $\frac{3}{4}$ of the air from the receiver, which was ſeen by the mercury riſing to the top of the tube; and upon opening the cock the mercury fell to its former ſtation, but then ſuddenly turning the cock, the mercury gradually roſe for the ſpace of 5 or 10 ſeconds to *more* than $\frac{1}{10}$ of its original height above the ſtationary point, and remained there till the cock was opened: after which it reſumed its proper ſtation.

Exp. 11. The manometer as before, but in rarefied air: The air let in ſhowed ſigns of giving out heat.

The phenomena in the two laſt experiments can be explained only on the following principle:—The air in the receiver and in the manometer is ſubject to a like degree of rarefaction and condenſation in thoſe experiments, or nearly ſo.

Fuller explanation. The air in the manometer recovers the common temperature ſooner

than that in the receiver: whence in the former cafe the gradual heating of the air in the receiver renders its preffure from elafticity greater on the mercury: and the contrary in the other cafe.

fo. When the equilibrium of heat in the air is difturbed by the operations of condenfation and rarefaction, it is reftored in the manometer *inftautly* by reafon of the contiguity of the glafs to the air; but in the large receiver it requires a fenfible time of 10 feconds or more to reftore the equilibrium throughout the whole internal capacity. It is this reftoration that increafes or diminifhes the elafticity of the air confined in the receiver, and thereby caufes the retrogradation of the mercurial column. Now I have found by former experiments, that a change of 50° in temperature effects a change of $\frac{1}{10}$ nearly, in the capacity or bulk of air. It follows therefore that in the cafe of reftoring the equilibrium in condenfed air, about 50° of cold is propuced; and in letting in air to an exhaufted receiver fomething more than 50° of heat is produced. The fmall difference feems to arife from this, that the condenfation of vapour in the former cafe *diminifhes* the effect, and in the latter, if any there be, *increafes* the effect, that would arife from operating upon purely dry air.

The theory of thefe effects refts on the difference of capacity of air for heat; viz. *lefs* the *greater the denfity.*

The experiments and obfervations hitherto related go principally to afcertain facts without any reference to the theory of them: This however may be given in a few words, and is the fame that is afcribed to Mr. Lambert by Meffrs. Sauffure and Pictet and by them adopted. He conceives that a vacuum has its proper capacity for heat, the fame as air, or any other fubftance; and that the capacity of a vacuum for heat is greater than that of an equal volume of atmofpherical air; alfo that the *denfer* air is, the *lefs* is its capacity for heat: upon thefe principles the phenomena are eafily referable to that clafs of chemical facts where heat and cold are generated by the mixture of two different bodies. If this theory be right, and I think there is little doubt of it, we may hence be led into a train of experiments, by which the abfolute capacity of a vacuum for heat may be determined; and likewife the capacities of the different gafes for heat, by a method wholly new: but this muft be left to future inveftigation.

Whence new experiments may be inftituted on the capacities of elaftic fluids.

VI. *An*

VI.

An Account of a Method of copying Paintings upon Glaſs, and of making Profiles by the Agency of Light upon Nitrate of Silver. Invented by T. WEDGWOOD, *Eſq. With Obſervations by* H. DAVY. *From the Journals of the Royal Inſtitution,* I. 170.

WHITE paper, or white leather, moiſtened with ſolution of nitrate of ſilver, undergoes no change when kept in a dark place; but, on being expoſed to the day-light it ſpeedily changes colour, and, after paſſing through different ſhades of grey and brown, becomes at length nearly black. White paper, &c. wetted with nitrate of ſilver, becomes black by light:

The alterations of colour take place more ſpeedily in proportion as the light is more intenſe. In the direct beams of the ſun, two or three minutes are ſufficient to produce the full effect. In the ſhade ſeveral hours are required, and light tranſmitted through different coloured glaſſes, acts upon it with different degrees of intenſity. Thus it is found that red rays, or the common ſun-beams paſſed through red glaſs, have very little action upon it: yellow and green are more efficacious; but blue and violet light produce the moſt decided and powerful effects*. with different ſpeed as the light is ſtronger; *e. g.* 2 minutes in ſunſhine. Coloured rays act with different intenſities.

The

* The facts above mentioned are analogous to thoſe obſerved long ago by Scheele, and confirmed by Senebier. Scheele found, that in the priſmatic ſpectrum, the effect produced by the red rays upon muriate of ſilver was very faint, and ſcarcely to be perceived; whilſt it was ſpeedily blackened by the violet rays. Senebier ſtates, that the time required to darken muriate of ſilver by the red rays, is 20 minutes, by the orange 12, by the yellow 5 minutes and 30 ſeconds, by the green 37 ſeconds, by the blue 29 ſeconds, and by the violet only 15 ſeconds.—*Senebier ſur la Lumière*, Vol. III. p. 199.

Some new experiments have been lately made in relation to this ſubject, in conſequence of the diſcoveries of Dr. Herſchel concerning the inviſible heat-making rays exiſting in the ſolar beams, by Meſſrs. Ritter and Bockmann in Germany, and Dr. Wollaſton in England.

It has been aſcertained by experiments upon the priſmatic ſpectrum, that no effects are produced upon the muriate of ſilver by the inviſible

Hence the light that paſſes through a painting on glaſs will cauſe various tinges on the paper, &c. and afford a copy;

The conſideration of theſe facts enables us readily to underſtand the method by which the outlines and ſhades of paintings on glaſs may be copied, or profiles of figures procured, by the agency of light. When a white ſurface, covered with ſolution of nitrate of ſilver, is placed behind a painting on glaſs expoſed to the ſolar light; the rays tranſmitted through the differently painted ſurfaces produce diſtinct tints of brown or black, ſenſibly differing in intenſity according to the ſhades of the picture, and where the light is unaltered, the colour of the nitrate becomes deepeſt.

or profiles may be taken from ſhadows.

When the ſhadow of any figure is thrown upon the prepared ſurface, the part concealed by it remains white, and the other parts ſpeedily become dark.

For copying paintings on glaſs, the ſolution ſhould be applied on leather; and, in this caſe, it is more readily acted upon than when paper is uſed.

The tinge is permanent.

After the colour has been once fixed upon the leather or paper, it cannot be removed by the application of water, or water and ſoap, and it is in a high degree permanent.

The copies thus made are changeable by daylight;

The copy of a painting, or the profile, immediately after being taken, muſt be kept in an obſcure place. It may indeed be examined in the ſhade, but, in this caſe, the expoſure ſhould be only for a few minutes; by the light of candles or lamps, as commonly employed, it is not ſenſibly affected.

and this imperfection has not yet been remedied.

No attempts that have been made to prevent the uncoloured parts of the copy or profile from being acted upon by light, have as yet been ſucceſsful. They have been covered with a thin coating of fine varniſh, but this has not deſtroyed their ſuſceptibility of becoming coloured; and even after repeated waſhings, ſufficient of the active part of the ſaline matter will ſtill adhere to the white parts of the leather or paper, to cauſe them to become dark when expoſed to the rays of the ſun.

Copies of textures thus made.

Beſides the applications of this method of copying that have been juſt mentioned, there are many others. And it

inviſible heat-making rays which exiſt on the red ſide, and which are leaſt refrangible, though it is powerfully and diſtinctly affected in a ſpace beyond the violet rays out of the viſible boundary. *See Annalen der Phyſik, ſiebenter Band,* 527.—D.

4 will

will be uſeful for making delineations of all ſuch objects as are poſſeſſed of a texture partly opaque and partly tranſparent. The woody fibres of leaves, and the wings of inſects, may be pretty accurately repreſented by means of it, and in this caſe it is only neceſſary to cauſe the direct ſolar light to paſs through them, and to receive the ſhadows upon prepared leather.

When the ſolar rays are paſſed through a print and thrown upon prepared paper, the unſhaded parts are ſlowly copied; but the lights tranſmitted by the ſhaded parts, are ſeldom ſo definite as to form a diſtinct reſemblance of them by producing different intenſities of colour. Prints not very diſtinct.

The images formed by means of a camera obſcura, have been found to be too faint to produce, in any moderate time, an effect upon the nitrate of ſilver. To copy theſe images was the firſt object of Mr. Wedgwood, in his reſearches on the ſubject, and for this purpoſe he firſt uſed the nitrate of ſilver, which was mentioned to him by a friend, as a ſubſtance very ſenſible to the influence of light; but all his numerous experiments as to their primary end proved unſucceſsful. The images of the camera are too faint for this proceſs.

In following theſe proceſſes I have found, that the images of ſmall objects, produced by means of the ſolar microſcope, may be copied without difficulty on prepared paper. This will probably be a uſeful application of the method; that it may be employed ſucceſsfully however, it is neceſſary that the paper be placed at but a ſmall diſtance from the lens. Thoſe from the ſolar microſcope are better.

With regard to the preparation of the ſolution, I have found the beſt proportions thoſe of 1 part of nitrate to about 10 of water. In this caſe, the quantity of the ſalt applied to the leather or paper, will be ſufficient to enable it to become tinged, without affecting its compoſition, or injuring its texture. To make the ſolution.

In comparing the effects produced by light upon muriate of ſilver, with thoſe produced upon the nitrate, it ſeemed evident, that the muriate was the moſt ſuſceptible, and both were more readily acted upon when moiſt than when dry, a fact long ago known. Even in the twilight, the colour of moiſt muriate of ſilver ſpread upon paper, ſlowly changed from white to faint violet; though under ſimilar circumſtances, no immediate alteration was produced upon the nitrate. The muriate is better than the nitrate as to delicacy,

The

but is lefs foluble.

The nitrate, however, from its folubility in water, poffeffes an advantage over the muriate: though leather or paper may, without much difficulty, be impregnated with this laft fubftance, either by diffufing it through water, and applying it in this form, or by immerfing paper moiftened with the folution of the nitrate in very diluted muriatic acid.

Caution.

To thofe perfons not acquainted with the properties of the falts containing oxide of filver, it may be ufeful to ftate, that they produce a ftain of fome permanence, even when momentarily applied to the fkin, and in employing them for moiftening paper or leather, it is neceffary to ufe a pencil of hair, or a brufh.

Something like a dying procefs may perhaps be effected here.

From the impoffibility of removing, by wafhing, the colouring matter of the falts from the parts of the furface of the copy which have not been expofed to light, it is probable that, both in the cafe of the nitrate and muriate of filver, a portion of the metallic oxide abandons its acid to enter into union with the animal or vegetable fubftance, fo as to form with it an infoluble compound. And, fuppofing that this happens, it is not improbable but that fubftances may be found capable of deftroying this compound, either by fimple or complicated affinities. Some experiments on this fubject have been imagined, and an account of the refults of them may poffibly appear in a future number of the Journals. Nothing but a method of preventing the unfhaded parts of the delineation from being coloured by expofure to the day is wanting, to render the procefs as ufeful as it is elegant.

VII. *Obfervations*

VII.

*Obſervations and Experiments upon Oxigenized * and Hyperoxigenized Muriatic Acid; and upon ſome Combinations of the Muriatic Acid in its Three States. By* RICHARD CHENEVIX, *Eſq. F. R. S. M. R. I. A. From the Philoſophical Tranſactions for* 1802.

WHEN Mr. Berthollet made known the combination of what was then called oxigenated muriatic acid with potaſh, he gave as his opinion, that the proportion of oxigen, relatively to the quantity of acid, was greater in the ſalt than in uncombined oxigenized muriatic acid. This conjecture was fairly founded upon the obſervation, that, in his mode of preparing this ſalt, a large portion of common muriate was formed in the liquor, along with the hyperoxigenized muriate. The memoir which he publiſhed in the year 1788, is the laſt with which I am acquainted, upon this ſubject. It does not contain any thing that, conſidering the accuracy which is now required in experiments, amounts to a demonſtration of the relative proportions of oxigen, in oxigenized and hyperoxigenized muriatic acids. Unfortunately, this chemiſt has not purſued his reſearches any farther; although, from his own words, we had every reaſon to hope that they would have been continued.

Inference of Berthollet, that the ſo called oxigenated muriate of potaſh contains more oxigen than is in the uncombined acid.

In the *Syſtème des Connoiſſances Chimiques* of Mr. Fourcroy, we find a ſummary of the experiments that had preceded the impreſſion of his work, together with the following ſentence: "Tous les muriates ſuroxigénés ſont décompoſés par les "acides, ſouvent avec une violente décrépitation, avec un "dégagement de vapeur jaune verdâtre, et une odeur très-"forte. Cette vapeur eſt de véritable acide muriatique ſur-"oxigéné. Elle eſt lourde, tombe en goutellettes d'un jaune "vert, et forme des ſtries comme de l'huile, ſur les corps aux-"quels elle adhère." This aſſertion carries no confirmation along

Fourcroy's account.

* I have preferred this word to *oxigenated*, becauſe *ate* is the appropriate termination of certain ſalts formed by acids in *ic*. Some further remarks upon this ſubject will be made in a work now in the preſs, intitled, *Remarks upon Chemical Nomenclature.*

along with it; and does not amount ſo near to proof as the poſition of the former chemiſt: ſo that, in fact, the exiſtence of hyperoxigenized muriatic acid, and of its combination with potaſh, reſts, at preſent upon the conjecture of Mr Berthollet; a conjecture however which, as well as his whole diſſertation upon the ſubject, bears all the marks of genius which ſo ſtrongly characteriſe every production of that ſagacious philoſopher.

The exiſtence of hyperox. mur. acid not yet proved.

Some notice has been taken of other ſaline combinations, formed by cauſing a current of oxigenized muriatic acid to paſs through ſolutions of the alkalis, or earths, or by otherwiſe combining them. Meſſ. D'Olfus, Gadolin, Van-Mons, Lavoiſier, and others, have ſlightly mentioned ſome of theſe combinations. But, with the exception of Mr. Berthollet, I know of no chemiſt who has approached ſo near to the real ſtate of the combination of muriatic acid and oxigen with potaſh, as Mr. Hoyle, of Mancheſter. The true nature of this ſalt, however, is one of thoſe things which many perſons have credited without proof; and which many others have been on the eve of diſcovering.

New obs. and expts. in proof of that ſtate.

I ſhall now proceed to lay before the Society, an account of the obſervations and experiments which have led me to conclude, that muriatic acid does exiſt in the form of oxigenized and hyperoxigenized muriatic acid, as announced in the title of the preſent communication; and that, in either ſtate, it is capable of entering into ſaline combinations.

Order of narrative.

With this view, I ſhall deſcribe,

1ſt, The means by which I think I have ſucceeded, in aſcertaining the conſtituent parts, as well as the proportions, in oxigenized and hyperoxigenized muriatic acid.

2dly, I ſhall mention ſome of the combinations of the muriatic acid, in its three ſtates.

In treating upon the firſt of theſe objects, I muſt in ſome meaſure anticipate the ſecond; and muſt ſuppoſe ſome things known, which are hereafter to be deſcribed. This inconvenience is inevitable; as the natural order of things leads me to treat of the acid, before I conſider the bodies into the compoſition of which it enters.

Hyperox. mur. of potaſh over a lamp decrepitated, melted, remained in fuſion, and loſt

I expoſed to the heat of a lamp, 100 grains of hyperoxigenized muriate of potaſh. It decrepitated gently, and in a ſhort time melted. After remaining in fuſion nearly an hour, I allowed it to cool; it cryſtallized as formerly, and had loſt

2,5 per

2,5 per cent. I increased the heat to redness, in a furnace. The salt boiled with a violent effervescence, and rapid disengagement of gaseous fluid, together with a thin white vapour, and then sunk suddenly into a white spongy mass. The loss of weight usually varied from 42 to 48 or 50 per cent.

0,025. Red heat caused escape of gas and loss of 0,5.

I put 100 grains into a coated glass retort, luted to a small and perfectly dry receiver, having a tube communicating with a glass bell in the pneumatic tub. The fire had not been lighted very long, when a slight dew began to line the inside of the receiver; and, as soon as the retort was nearly red-hot, a disengagement of gas, so sudden as almost to be explosive, took place. A quantity of thin white vapour arose, which afterwards was deposited, in the form of a white sublimate, in the receiver and the tube. When the extrication of gas had ceased, the apparatus was allowed to cool. The gas, with the usual corrections of temperature and pressure, measured 112,5 cubic inches, = 38,3 grains. The 2,5 mentioned above, as the loss of this salt at a low heat, were water. 53,5 remained in the retort; and the white sublimate in the tube and receiver amounted to 5.

Distillation of hyperox. mur. of potash, afforded oxigen; and left muriate.

The products of this operation were therefore,

Water - - - -	2,5
Oxigen - - - -	38,3
Salt in the tube and receiver - -	5
Salt remaining in the retort -	53,5
	99,3.

To find the proportions of oxigen and muriatic acid, in hyperoxigenized muriatic acid, it now only remains to determine the sum of the quantities of muriatic acid, contained in the 53,5 of the retort, and the 5 of the tube and receiver. The 53,5 gave, by nitrate of silver, a precipitate corresponding to 18,21; and the 5, a precipitate corresponding to 1,76; in all, 20 of the muriatic acid. Therefore, 38,3 of oxigen, and 20 of muriatic acid, combine to form 58,3 of hyperoxigenized muriatic acid; or, 100 of hyperoxigenized muriatic acid contain, within a fraction,

The residual muriate was employed to precipitate nitrate of silver, and its quantity of mur. acid inferred from the precipitate.

Oxigen - - - -	65
Muriatic acid - - -	35
	100.

And

The hyperox. muriate therefore contained nearly 1 part common acid, 2 parts potash, and 2 parts oxigen.

And the elements of hyperoxigenized muriate of potash, should be thus stated :

Oxigen - 38,3	hyperoxigenized muriatic acid	-	58,3
Muriatic acid 20			
	Potash	- -	39,2
	Water	- -	2,5.
			100,0.

Muriates lose a little acid by ignition.

It may be observed, that the 53,5 of the retort did not yield the same proportion of acid as the 5 of the tube and receiver. The fact is, that all muriates lose a little of their acid at a red heat, as I shall presently mention more particularly ; and the small loss was, in all probability, owing to a portion of acid disengaged by the heat to which the salt was necessarily exposed during the operation.

To determine the oxigen added to convert muriatic into oxyg. mur. acid.

Having thus ascertained the proportion of oxigen in hyperoxigenized muriatic acid, by means of its combination with potash, a ready method occurred to arrive at the knowledge of that contained in oxigenized muriatic acid. For this purpose, I disposed in the following manner, a Woulfe's apparatus, consisting of three bottles, and connected with the pneumatic tub. In the first bottle, I put a solution of potash *, in about six parts of water. In the second, a solution of the same ; but so dilute, as that no part of the salt, which might be formed, should crystallize during the operation. About twenty parts of water was the proportion there employed. In the third bottle, I put common carbonate of potash. Through this apparatus, I sent a current of oxigenized muriatic acid, disengaged by sulphuric acid, from a mixture of muriate of soda and black oxide of manganese, in the well known manner. Crystals of hyperoxigenized muriate of potash were formed in the liquor of the first bottle ; and, as long as they remained, I was certain, from previous experiment, that no sulphuric or muriatic acid could pass into the second bottle. The current was continued, till the liquor of that bottle contained an excess of acid. The carbonate of potash, in the third bottle, absorbed the superabundant vapours ; and the pneumatic apparatus was ready to collect any gases that might be evolved. By these means, I

Ox. mur. acid gas was passed in succession through strong solution of potash, weak solution of potash, and solution of carbonate of potash.

The first bottle exhibited crystals and detained all the sulph. or mur. acid.

The second bottle received only hyperox. mur. acid, and contained its dissolved combination with potash.

* Whenever potash, soda, barytes, an acid, an alkali, water, or the names of other substances are used without an epithet, they are meant to denote them in that state which is commonly called *pure.*

obtained

obtained, in the ſecond bottle, a ſolution of whatever ſubſtance might reſult from the action of potaſh upon hyperoxigenized muriatic acid.

The third bottle abſorbed the ſuperabundant vapours.

I took a portion of this liquor, which I ſhall call *entire liquor* *, and diſtilled it to dryneſs in a glaſs retort, taking care to ſcreen it from light. A tube from the receiver communicated with the pneumatic tub. My object was to aſcertain, whether the change obſerved by Mr. Berthollet, in the diſtribution of the elements of oxigenized muriatic acid, to form, with potaſh, a ſimple and a hyperoxigenized muriate, really took place among thoſe elements themſelves, independently of any abſorption of oxigen from the atmoſphere, or extrication of it from the ſalt. Nothing but ſome water, and a few inches of the dilated air of the veſſels, paſſed into the receiver and the pneumatic apparatus; and I found, in the retort, a ſaline maſs †, perfectly dry and cryſtallized. Hence it is evident, that the ſame quantity of oxigen as that formerly contained in the oxigenized muriatic acid, which had been united to the alkali, to form the total maſs of ſalt, was now condenſed, in that part which had become hyperoxigenized muriate.

The liquor of the ſecond bottle gave no gas by diſtillation, but left the dry cryſtallized ſalt.

To aſcertain this quality, I diſſolved 100 grains of the entire ſalt in water, and precipitated by nitrate of ſilver. I thus obtained a quantity of muriate of ſilver, which, by proportions previouſly determined, I knew to correſpond to 84 of muriate of potaſh: therefore, 16 were hyperoxigenized muriate of potaſh ‡. But, according to the proportions eſtabliſhed above in hyperoxigenized muriate of potaſh, 16 of this ſalt contain 6 of oxigen, with 3,20 of acid, the remainder being alkali and water; and, by preliminary experiments, I found that 84 of muriate of potaſh contained 27,88 of muriatic acid. Therefore $27,88+3,20=31,08$ of muriatic acid, with 6 of oxigen, or, to reduce it to the quintal,

100 grains of this ſalt precipitated as much ſilver as 84 grains of muriate of potaſh would have done.

Therefore 16 parts were hyperoxigenized muriate.

* I am well aware that, upon philoſophical principles, this appellation is objectionable; but, for the ſake of brevity, I have uſed it as a temporary name for a ſubſtance which has only a relative exiſtence among chemical bodies.

† This ſalt I ſhall call *entire ſalt*.

‡ I muſt obſerve here, that hyperoxigenized muriate of potaſh does not, like ſimple muriate, decompoſe the ſalts of ſilver. This ſhall be further animadverted upon, and proved, in its proper place.

Muriatic

Muriatic acid - - -	84
Oxigen - - - -	16
	100,

are the proportions which combine to form oxigenized muriatic acid.

Confirmation by distilling off the oxigen.

To corroborate this evidence, I distilled 100 grains of the intire salt mentioned above; and obtained nearly 16,5 cubic inches of oxigen gas; which as accurately corresponds with the trials by nitrate of silver, as can be expected in experiments of this nature.

Comparison with the experiments of Berthollet;

Mr. Berthollet, in his memoir upon oxigenized muriatic acid, gives, if I understand him rightly, the following statement of the proportions, and of the means by which he obtained his results. He exposed to the light of the sun, 50 cubic inches of water, saturated with oxigenized muriatic acid; and collected in the pneumatic tub, 15 cubic inches of oxigen gas. I here neglect fractions; because our results appear, at first sight, to differ so widely as not to require great accuracy in giving their comparative statement. He then precipitated, by nitrate of silver, the 50 cubic inches of liquor, which had become simple muriatic acid, and obtained 383 grains of muriate of silver. But, by experiments, I found that 383 grains of muriate of silver contain 65 of muriatic acid. Therefore, 65 of muriatic acid combine with 15 cubic inches * (= 8 grains) of oxigen, to form 73 of oxigenized muriatic acid. But 73 : 8 : : 100 : 11, or nearly. For this difference, however, it may be easy to account. Perhaps Mr. Berthollet's 50 cubic inches of oxigenized muriatic acid, contained originally a little simple muriatic acid; and he says besides, that he suspects all the oxigen was not disengaged. This indeed is most probable; and I am happy that I can reconcile the proportions which I have found, to the opinion of so skilful a chemist.

and Cruickshank.

Mr. Cruickshank likewise, in his additional Observations upon Hydrocarbonates, has stated that 2,3 parts of oxigenized muriatic acid contain 1 of oxigen, or about 43,5 per cent. But this able chemist, to whom we are indebted for the discovery of the gaseous oxide of carbon, procured his oxigenized muriatic

* Mr. Berthollet's proportions are in the old French weights and measures.

acid

acid by a peculiar method, which I ſhall notice in ſpeaking of the action of acids upon hyperoxigenized muriate of potaſh. The ſubſtance he obtained was, in fact, not oxigenized muriatic acid gas, but a mixture of that gas with hyperoxigenized muriatic acid. I have not the ſmalleſt doubt of the accuracy of his ſtatement; but, being the proportion of a mixture, it in no way contradicts either of thoſe I have determined in this Paper.

There was no effervescence or loſs of oxigen in the experiments.

Before I diſmiſs this part of the ſubject, I wiſh to anticipate an objection, founded upon an obſervation of Mr. Berthollet, which may be made to the above experiments. He ſays, that when the alkaline ſolution is very concentrate, an effervеſcence takes place durin thewhole of the ſaturation, and for ſome days after; and this effervеſcence he attributes to the eſcape of oxigen. But I have already ſaid, that no oxigen gas was diſengaged in any part of my proceſs; and no effervеſcence took place in any of the bottles, except the third; ſo that no ſuperabundance of oxigen could have paſſed from one into the other, nor could any diminution of the total quantity have been produced. By repeating the experiments, ſometimes with a ſolution of alkali, and ſometimes with water alone, in the firſt bottle, I obtained the liquor in the ſecond bottle uniform in all caſes. Indeed, as potaſh prepared in Mr. Berthollet's manner, was not in ſuch general uſe at the time he performed his experiments as at preſent, I ſuſpect that a great part of this effervеſcence was owing to a diſengagement of carbonic acid from the alkali.

It is probable the effervеſcence of Berthollet was owing to carbonic acid.

Having thus proved the difference between the ſtates of theſe two acids, I ſhall now proceed to the combination of each with ſalifiable baſes.

OXIGENIZED MURIATES.

Whether the liquor before evaporation did contain the two ſalts or only one oxigenized ſalt, before the inſtant of cryſtallization.

As many properties of the entire liquor, before it had been evaporated to dryneſs, had led me to imagine that the acid was united with the alkali, and remained in combination with it, in the ſtate of oxigenized muriatic acid, till the moment of cryſtallization, I think it neceſſary to ſtate at length the appearances which induced me to draw that concluſion, and the experiments which afterwards convinced me that it was erroneous.

Sulphuric or acetic acid difengaged oxyg. m. acid from the liquor; but not from the falt.

A few drops of fulphuric acid, poured into fome of the intire liquor, caufed an effervefcence, and a fmell of oxigenized muriatic acid.

Very ftrong acetic acid produced the fame effect.

This however arifes from fome difengaged excefs of oxyg. mur. acid; for neutral falts could extricate it.

By other experiments, I had afcertained that acetic acid could not decompofe any part of the intire falt; and hence I concluded, that in the intire liquor, before evaporation, fome of the falt remained in the ftate of oxigenized muriate, the acid of which was expelled by the fulphuric or acetic acid; and, that it was not till the moment of cryftallization that the elements of the falt underwent a total refolution into muriate, and hyperoxigenized muriate of potafh. However, a fmall quantity of any of the very foluble neutral falts, fuch as nitrate or muriate of ammonia, or even a little alcohol, produced the fame effects; and I was then convinced, that the effervefcence was owing to fome uncombined oxigenized muriatic acid gas remaining in the liquor; and which was difengaged, in proportion as the water was taken from it, by the fuperior affinity of the falt, or the alcohol, I had ufed.

Acetic and acetous acids do not decompofe hyperox. m. of potafh; but ox. m. acid decompofes acetite of potafh.

By fome previous experiments I had afcertained, as I have juft mentioned, that acetic or acetous acids do not decompofe hyperoxigenized muriate of potafh. I fent a current of oxigenized muriatic acid through a folution of acetite of potafh; and, upon evaporation, I found that the acetous acid had been difengaged, and that muriate, with hyperoxigenized muriate, of potafh had been formed. But, from fome trials, which I fhall prefently relate, I was induced to believe, that oxigenized muriatic acid attracts the falifiable bafes with a much weaker affinity than acetous acid. It is well known that the contact of oxigenized muriatic acid with an alkali, is fufficient to produce a combination of that acid with the alkali; and, from the laft-mentioned experiments it appears, that it is not abfolutely neceffary that the alkali fhould be in a free ftate. If it be combined with an acid weaker than hyperoxigenized muriatic acid, the original acid will be expelled; and muriate and hyperoxigenized muriate will be formed, as if the alkali had been free.

In fuch difengagements the alkali becomes combined as if it had at firft been free.

As a further proof, that the change in the diftribution of oxigenized muriate of potafh takes place at the moment of contact of the acid and the alkali, and confequently long before the cryftallization, I mention the following experiments.

I pre-

I precipitated, by nitrate of filver, 400 grains of the entire liquor, previoufly to its being evaporated; and obtained 71 grains of muriate of filver.

The firft liquor was precipitated by nitrate of filver;

I evaporated to drynefs, 400 grains of the fame liquor, redif-folved the refiduum, and, by dropping in nitrate of filver, ob-tained 70 grains of muriate. The difference of one grain, in thefe experiments, does not amount to 0,2 of a grain of muriate of filver; and ought not to be regarded.

and afforded the fame refult as a folution of the intire falt.

From thefe experiments it is paft all doubt, that the original intire liquor did not contain oxigenized muriate of potafh. For, if fuch a combination had exifted in it, I fhould have obtained a fmaller portion of muriate of filver in the firft than in the fecond cafe, on account of the total feparation into muriate and hyperoxigenized muriate having not yet taken place.

Hence the firft liquor did not contain oxig. m. of potafh; but the two diftinct falts.

We are not however to conclude, from thefe experiments, that there are no fuch things as oxigenized muriates. Although they cannot be exhibited in a palpable ftate, it is eafy to de-monftrate that they do really exift. I fhall prove, in the pro-per place, that hyperoxigenized muriate of ammonia is not an incompatible combination; and muft, for the prefent, af-fume the datum, in order that I may demonftrate the neceffary exiftence of oxigenized muriates. Therefore: If muriatic acid, or if hyperoxigenized muriatic acid, be brought in con-tact with ammonia, the refult will be muriate, or hyperoxigen-ized muriate, of ammonia. But, if the acid, difengaged by fulphuric acid, from a mixture of black oxide of manganefe and muriate of foda, be fent through ammonia, both are decom-pofed. Hence it is evident, that the acid combines with the alkalis, in the ftate of oxigenized muriatic acid; and that the feparation into muriate and hyperoxigenized muriate, is pro-duced by a fubfequent action, among the elements of oxigen-ized muriate of potafh.

Yet it is poffible to form oxig. muriates, for example, of ammonia.

For the oxigen-ized acid can de-compofe ammo-nia, and the hy-perox. m. acid does not; and this laft does, in fact, form a falt with it in the experiment.

Upon the whole, it appears to me fair to conclude,

1ft, That the falts of this genus do really exift, previous to the formation of hyperoxigenized muriate of potafh.

2d, That the affinity exercifed by hyperoxigenized muriatic acid for ammonia, and (by very ftrong analogy) for the other bafes, is much greater than that of oxigenized muriatic acid. For, hyperoxigenized muriatic acid, as fhall prefently be fhewn, having a much more powerful action upon all combuftible bodies,

Confequently there muft have been an oxig. muriate firft, and this was imme-diately, by ftronger affinity, fucceeded by the

other falt, inftead of decompofition enfuing.

bodies, whether fimple or compound, than oxigenized muriatic acid, it would be natural to fuppofe that the former acid would act more powerfully upon the inflammable element of ammonia. But oxigenized muriatic acid combines with the hidrogen of that alkali; which, however, is not decompofed by hyperoxigenized muriatic acid; yet the affinity of hyperoxigenized muriatic acid for ammonia, is the only caufe that determines the union of the acid and the alkali, without decompofition. But thefe affinities fhall be more fully treated of, in fpeaking of hyperoxigenized muriate of ammonia.

ALKALINE AND EARTHY HYPEROXIGENIZED MURIATES.

Generic Characters.

Alk. and earthy hyperox. muriates. Generic characters.

Hyperoxigenized muriates are formed by paffing a current of oxigenized muriatic acid through the bafis, diffolved or fufpended in water, as in the formation of the laft-mentioned genus. The firft formation is owing to the feparation of the elements of an oxigenized muriate, into hyperoxigenized muriate and fimple muriate; from which latter, they may be feparated by chryftallization, or by another procefs, which I fhall mention, in treating of the earthy hyperoxigenized muriates. By fimple trituration, they fcintillate, with noife. They are decompofed by a low red heat; and give out a confiderable quantity of oxigen, as they become fimple muriates. They cannot be brought down, by any means that I have tried, to that diminifhed ftate of oxigenizement, which would conftitute oxigenized muriates. They inflame all combuftible bodies with violence, as is well known. They are foluble in water; many of them, in alcohol; and fome are deliquefcent. The acid is expelled, with particular phenomena, by fulphuric, nitric, and muriatic acids, without heat; and, a little below a boiling heat, by phofphoric, oxalic, tartareous, citric, and arfenic acids: but they are not acted upon by benzoic, acetic, acetous, boracic, pruffic, or carbonic acids. Thofe vegetable acids which are powerful enough to decompofe them, give out, towards the end, a gas of a peculiar nature, which has not fo much fmell as oxigenized muriatic acid gas, but which affects the eyes in an extraordinary manner, and promotes an uncommon

uncommon and rather painful fecretion of tears. I have not yet examined this gas, as there was invariably an inflammation of the mixture, with explofion and rupture of the veffels, almoft as foon as it began to be evolved. When pure, the hyperoxigenized muriates do not precipitate any of the metallic falts, although I believe they decompofe fome. The order in which the bafes feem to be attracted by the acid, is, potafh, foda, barytes, ftrontia, lime, ammonia, magnefia, alumina, filica. The other earths I have not tried, and but few of the metallic oxides.

Firft Species. Hyperoxigenized Muriate of Potafh.

This falt is the beft known of all the faline combinations of this acid. It has been erroneoufly confidered as fimply oxigenized, for its acid is really hyperoxigenized. It is foluble in about fixteen parts of cold water, but in much lefs of warm; and is eafily feparated, by cryftallization, from muriate of potafh. Alcohol can diffolve a fmall portion of it. It feems capable of exifting in more ftates than one; for, in paffing a current of oxigenized muriatic acid, very flowly, and in the dark, through a folution of potafh, till faturated, I have obtained flexible and fhining needle-like cryftals. This leads me to fufpect, either a hyperoxigenized muriate of potafh with excefs of acid, or that acid with a fuperaddition of oxigen. It would be fuperfluous to enter into a minute defcription of a fubftance fo well known as hyperoxigenized muriate of potafh; but, it being the fubftance whence I have chiefly attempted to difengage the acid, I fhall enter into a particular detail of the action of the more powerful acids upon this falt.

Hyperox. mur. of potafh.

Probably capable of exifting in various ftates.

If concentrate fulphuric acid be poured upon hyperoxigenized muriate of potafh, a violent decrepitation, fometimes but rarely accompanied by a flafh, takes place. A thick heavy vapour, of a greenifh yellow colour, which rifes with difficulty to the top of the veffel, if it be deep, is difengaged. The fmell is not altogether unlike nitrous gas; but it is peculiarly fetid, and may be compared to that which is emitted by brick-kilns, mixed with that of nitrous gas. It differs much from oxigenized muriatic acid gas; the latter being pungent and penetrating, the other heavy and oppreffive; and it does not produce, at leaft in fo great a degree, the catarrhal fymptoms caufed by the other. At the bottom of this vapour is a bright orange-

Attempt to decompofe it by fulph. acid added to the falt.

Peculiar vapour.

The hyperox. m. acid thus disengaged is not pure.

orange-coloured liquor, which has the fame fmell as the vapour. This is the acid contained in the falt; and I have proved it to be hyperoxigenized muriatic acid. But, although the falt from which the acid is difengaged be pure, the acid itfelf is never fo; becaufe the very act of difengaging it effects its decompofition, and fome of it is converted into oxigenized muriatic acid. The colour of litmus paper, on this account, is generally deftroyed by the liquor. I fay on this account, becaufe I have fome reafon to believe, from having obferved this not to be uniformly the cafe, that hyperoxigenized muriatic acid reddens the vegetable blues. However, it muft be confidered, that the fulphuric acid ufed to difengage the hyperoxigenized muriatic acid is ftill prefent; and we can draw no certain conclufion, until we have obtained this acid free from all other fubftances. If to this mixture of hyperoxigenized muriate of potafh and fulphuric acid, heat be applied, an exceedingly violent explofion, with a white and vivid flafh, takes place, before the liquor has attained the temperature of 125 of Fahrenheit. In order to obtain this acid, I attempted to diftil 500 grains, in a glafs retort, in a water bath, with every precaution againft fuch accidents as I could not but in fome meafure expect; when, almoft as foon as I had kindled the fire, I faw, in the bottom of the retort, an extremely white, vivid, and rapid flafh, which was immediately followed by a loud report. The retort was reduced almoft to powder; fo that fcarcely any fragments of it could be found in the laboratory. The windows, and feveral glafs veffels, were broken. I happened to be holding the neck of the retort, at the moment of the explofion, yet received no injury, except a flight contufion in the hand. But Dr. Vandier, a French gentleman of confiderable chemical and medical talents, to whom I am indebted for much able affiftance in my laboratory, was wounded in feveral places; particularly, the tunica conjunctiva of the eye was fo lacerated, that a piece of it hung down, and, by getting under the inferior eyelid, caufed the moft painful irritation, and endangered his fight. One of the frontal arteries alfo was divided. I relate thefe circumftances thus fully, as the moft effectual means of putting upon their guard thofe who would repeat the experiment. If the fulphuric acid be dilute, heat may be applied with more fafety; and the phenomena are different. The hyperoxigenized muriatic acid is difengaged

Heat produces violent and dangerous explofion.

Narrative of an accident of this nature.

Dilute acid applied.

gaged from the bafis; but, as the heat requifite to diftil the acid is more than fufficient to decompofe it, oxigenized muriatic acid comes over with it; and oxigen gas is collected in the pneumatic tub. If the diftillation be continued, the fame danger arifes as in the former cafe, becaufe the fulphuric acid becomes concentrated; and it would feem, that its action upon the falt is flight and partial at a low temperature, but violent and inftantaneous when heated and concentrate. I could not, therefore hope, by thefe means, to obtain the acid difengaged and pure.

If the manner of bringing the fulphuric acid and the falt into contact be reverfed, and the falt be dropped into the acid, the yellow vapours and the orange-coloured liquor are produced, but generally without decrepitation. If they be allowed to remain fome days in contact, the vapours continue, and oxigen gas is conftantly difengaged, even in the common light of the day, and at the temperature of the atmofphere. By cooling the firft receiver with ice, I thought that I had once obtained this acid cryftallized in the form of four-fided pyramids, of an orange colour. But, though I really believe this to have been the cafe, I do not pofitively affirm it. The falt added to the acid.

Nitric acid produces nearly the fame phenomena; but the fmell and other properties are rather lefs diftinct and marked, than with fulphuric acid. Nitric acid applied.

Muriatic acid decompofes this falt, and unites to its bafis; but neither the yellow vapours, nor the orange-coloured liquor, are produced. The circumftances which attend the contact of the acid and the falt, are as follows. If no more muriatic acid be prefent than is merely neceffary to decompofe the falt, I do not doubt that hyperoxigenized muriatic acid will be driven off, as little decompofed as with the other acids, fuppofing the action to be inftantaneous; but, during the contact of thefe two bodies, the acid expelled muft meet muriatic acid not yet combined, and, uniting with it, always forms a portion of oxigenized muriatic acid. The quantity of the laft acid muft vary, according to the quantity of muriatic acid employed, and not combined with the alkali. It was by this method that Mr. Cruickfhank obtained the muriatic acid gas, which he ftated to contain 43,5 per cent. of oxigen. Muriatic acid.

Phofphoric and arfenic acids do not act upon this falt, till heated with it; and then much oxigen gas is evolved. Thefe, therefore, Phofph. and acetic acids difengage oxigen by heat.

therefore, afford no better method of diſengaging hyperoxigenized muriatic acid without decompoſition.

Oxalic, tartareous, and citric acids.

Oxalic, tartareous, and citric acids, act as I before mentioned; and the hyperoxigenized muriatic acid holds its place, in the order of affinities for potaſh, immediately before the benzoic.

Amuſing combuſtions by this ſalt and ſtrong acids.

I ſhall not ſtop to detail a number of amuſing phenomena that may be produced, by projecting into the ſtronger acids, mixtures of combuſtible bodies, whether metallic or not, and hyperoxigenized muriate of potaſh. The cauſe of them is well underſtood, and the theory points them out: they are, therefore, no longer objects of philoſophical admiration. But I muſt mention one experiment, which, had it ſucceeded, I ſhould have thought important. I projected various mixtures of very minutely pulverized diamond and this ſalt, into the different acids; but found the diamond undiminiſhed, by every attempt to combine it with oxigen in the humid way *.

The diamond does not burn in this humid way.

Another, but imponderable, part of this ſalt, as indeed of all hyperoxigenized muriates, ſeems to be an extraordinary quantity of caloric. For, during their formation, ſcarcely any heat is diſengaged, as by other acids; and, very little heat applied to the ſalts, gives the gaſeous form to their oxigen.

The h. ox. muriates contain much caloric.

An opinion has prevailed among ſome ingenious chemiſts, that, from a mixture of this ſalt with ſulphuric acid, nitrous gas is diſengaged, and ſulphate of lime formed in the retort. But this is a miſtake, ariſing, on the one hand, from the ſmell and vapour of the hyperoxigenized muriatic acid, and, on the other, from ſulphate of lead, which the common ſulphuric acid of this country frequently contains in ſolution, and which is precipitated from it by water. Before we aſſert a fact, we ſhould be well aſſured of the pureneſs of our chemical agents. This

Erroneous opinion, that nitrous gas may be formed from this ſalt, &c.

* I muſt confeſs, that the vivid flaſhes of light, emitted from the mixture of this ſalt and combuſtible bodies thrown into an acid, appear to me, in ſome meaſure, to prove the modification propoſed by Leonhardi, Richter, Gren, &c. to that part of the Lavoiſierian theory which regards the emiſſion of light during combuſtion. Another teſtimony in favour of their modification, may be drawn from the vegetable kingdom. All plants growing in places deprived of light, are merely mucilaginous. But the mucilage of theſe plants burns without the emiſſion of light. Light, therefore, appears not to be diſengaged from oxigen; elſe, why not by this mucilage as well as by other combuſtible bodies?

ſuppoſed converſion of muriatic or hyperoxigenized muriatic acid into nitrous gas, will not paſs for a decompoſition, or a tranſmutation, of that refractory radical; and the idea of the change of potaſh into lime, is as erroneous as ſome other late aſſertions reſpecting the decompoſition of the alkalis.

The proportions of this ſalt are, as I before ſtated,

Component parts of hyp. ox. mur. of potaſh.

Hyperoxigenized muriatic acid	-	58,3
Potaſh - - - -	-	39,2
Water - - - -		2,5
		100,0.

Second Species. Hyperoxigenized Muriate of Soda.

Hyper ox. mur. of ſoda. Very difficult to be ſeparated, becauſe of the little difference of ſolubility.

This ſalt is prepared in the ſame manner, and with the ſame phenomena, as the former. It is extremely difficult to obtain it pure, as it has nearly the ſame degree of ſolubility in water as muriate of ſoda. It is ſoluble in three parts of cold, and leſs of warm water; and is ſlightly deliqueſcent. It is ſoluble in alcohol; but this property alone is not ſufficient to enable us to obtain it free from the muriate of ſoda, formed along with it in the entire liquor; as the latter ſalt, contrary to the aſſertions of all authors, is ſoluble in alcohol, and ſeems to be much more ſo, when accompanied by the hyperoxigenized muriate. It was by taking a large quantity of the entire ſalt, formed by ſending a current of oxigenized muriatic acid gas through a ſolution of carbonate of ſoda, and repeatedly cryſtallizing in alcohol, that, with great difficulty, I obtained a little pure hyperoxigenized muriate of ſoda. It cryſtallizes in cubes, or in rhomboids little different from cubes. It produces a ſenſation of cold in the mouth; and its taſte is eaſily diſtinguiſhed from muriate of ſoda. It is decompoſed by heat, by combuſtible bodies, and by acids, in the ſame manner as the former ſpecies; and the acid holds its place for ſoda, as for potaſh, immediately before the benzoic. The baſis is ſeparated by potaſh only. This ſalt is compoſed of,

Obtained by cryſtallizing in alcohol.

Component parts.

Hyperoxigenized muriatic acid	-	66,2
Soda - - - -	-	29,6
Water - - - -		4,2
		100,0.

(*The Remainder in our next.*)

VIII. *Deſcription*

VIII.

Description of the Indian Hand Mill. From a Correspondent.

General account of the hand mill.

THE Indian hand mill, like moſt other Indian inventions, is characteriſed by conſiderable ſimplicity and effect. Its cheapneſs and its general utility are ſuch, that the meaneſt hut in India is never without one. The whole of the grain uſed by the natives of India is ground in theſe mills, and chiefly by the women, who appear to execute the taſk with aſtoniſhing eaſe. A woman will continue grinding with this mill for ſeveral hours, and in this time ſhe will reduce a very conſiderable quantity of grain into flour.

Deſcription.

The mill is compoſed of two circular ſtones, an upper and an under one, each about two feet in diameter, and $2\frac{1}{2}$ inches in thickneſs. The quality of the ſtone nearly reſembles that of our common mill ſtones.

Figure and fitting of the under ſtone.

The upper face of the under ſtone is dreſſed perfectly flat. A ſmall hole, of nearly an inch in diameter, is ſunk in the center of this ſtone: this hole is fitted with a plug of hard wood, firmly driven into it, which contains a ſmall ſteel gudgeon or pivot. The ſurface of this wooden plug is ſlightly elevated above the face of the ſtone, and the gudgeon projects about half an inch beyond it. This gudgeon fits into a hole in the middle of a croſs bar, or flat piece of iron, eight or nine inches in length, about $\frac{3}{4}$ of an inch in breadth, and $\frac{1}{4}$ of an inch in thickneſs: the ends of this croſs bar are let into notches in the upper ſtone, and it thus forms a center on which the upper ſtone may revolve.

The upper ſtone.

The lower face of the upper ſtone is dreſſed perfectly flat, to correſpond with the upper face of the under ſtone. This ſtone is $2\frac{1}{2}$ inches in thickneſs at the edges, but it has a projection of nine inches diameter in the middle, at which place it is five inches in thickneſs. The ſtone is pierced in the middle by a hole of ſix inches in diameter, which ſerves as a hopper to contain the grain. In the upper face of this ſtone a hole is ſunk near the circumference, in which hole a wooden handle, about ſix inches in length, is firmly driven. By means of this handle the ſtone may be made to revolve on its gudgeon with conſiderable facility.

The

The projection of the wooden plug in the under ſtone in which the gudgeon is inſerted is ſo regulated as to keep the ſtones at the proper diſtance for forming the fineſt flour: if a coarſer ſort is wanted, it is readily obtained by placing one or more ſlips of paper or card in the notches of the upper ſtone, between the croſs bar and the ſtone. How the fineneſs of the flour is regulated.

The under ſtone is uſually placed on the earthen floor of the hut, which is previouſly beaten and ſwept, or otherwiſe it is placed on a cloth ſpread on the floor. As the grain is reduced to flour it eſcapes, together with the bran, at the circumference of the ſtones, and is collected on the floor or on the cloth. Local ſituation or poſition.

I would ſuggeſt as an improvement of this ſimple machine, the forming the croſs bar with three claws, which would give three points of bearing inſtead of two, and conſequently render the upper ſtone more ſteady *. I would alſo ſuggeſt, that the block of wood which contains the gudgeon, ſhould paſs completely through the under ſtone; thus it might eaſily be adapted to the requiſite height by a few blows of the mallet. Should it be thought neceſſary, the ſtones might be encloſed in a drum, and fitted on a ſtand. Improvements ſuggeſted.

Explanation of the ſketch, Plate X. Reference to the figures.

Fig. 1. Repreſents the lower face of the upper ſtone.

Fig. 2. Repreſents the upper face of the ſame.

Fig. 3. Is an elevation of the mill.

Fig. 4. Is a ſection of the mill.

a, Is the croſs bar, which is repreſented with three claws.

b, Is the handle by which the upper ſtone is turned.

c, Is the ſteel gudgeon on which the upper ſtone revolves by means of the croſs bar.

d, Is the ſmall block of hard wood in which the gudgeon is fixed.

* It is likely that this ſteadineſs would do harm. The totter in all probability anſwers the ſame purpoſe, of pounding, as the vertical dancing motion of the upper mill ſtone upon the ſupport of its pivot, of which Deſaguliers has given a very rational account. Lectures II. 428.

IX. *An*

IX.

An Effect hitherto unpublished, by which the two Electricities are distinguished. In a Letter from Mr. JOHN CUTHBERTSON.

To Mr. NICHOLSON,

DEAR SIR,

Letter.

ON reading your note respecting the instruments by which the two kinds of electricity are distinguished, or its direction ascertained, it brought to my mind an experiment which I made with that view in the year 1777 in Holland; I believe it is not known here, so perhaps you may think it worthy of a place in your valuable Journal.

I am,

Dear SIR,

your's respectfully,

J. CUTHBERTSON.

Poland Street, Soho, Oct. 19, 1802.

Experiment. A candle between two balls in the opposite states of electricity has its flame much agitated, and heats the negative ball only.

INSULATE two wires furnished at each end with a metal ball of about $\frac{3}{4}$ of an inch diameter, connect one with the positive conductor, and the other with the negative conductor of an electrical machine; set them so that their balls may be at about four inches distance, place between them a common sized lighted candle, with the center of its flame nearly upon a level with the centers, and at an equal distance from each; if the machine be put in motion the flame will waver very much, and seems rather more to incline to the negative ball than to the positive one, but is very equivocal: continue turning (if the machine be a plate of two feet diameter) about 50 revolutions, then the negative ball will begin to grow warm, and the positive ball remain cold; if the revolutions be multiplied to 200, the negative ball will be too hot to be touched, and the positive remain as cold as at the beginning.

X. *Palpable*

X.

Palpable Mathematics; or Methods of writing and calculating for the Use of the Blind. By J. B. BERARD.*

CITIZEN Haüy having compared the methods of writing and calculating of several celebrated blind men, has digested into a body of doctrine the best productions of experience in this art. He has composed a system of instruction, and has described the instruments proper to be used in this operation: and he truly deserves well of humanity, who increases the means of communication between society, and a class of men who seem to have been excluded from it, and to have been condemned to lead an unhappy life, useless to others, and without enjoyment to themselves.

Citizen Hauy's instructions to the blind as to writing, &c.

Having been myself deprived of sight since the age of twenty-three, meditation and experience have given me the unhappy right of treating upon the art concerning which he has written. I here give some observations which may not be entirely useless to society.

The author is blind.

ARITHMETIC.

The method by which the celebrated Saunderson calculated is well known; however ingenious it may be, it does not seem to me to be the best; I observe two principal defects in it.

Professor Saunderson's method of computing

He used small square pieces with nine holes in each: the position of a pin in one of these holes denoted one of the nine figures. It is therefore necessary to have as many of these squares as the operation requires of figures or characters: and we must employ an extended space for a very simple calculation.

by squares to denote the digits,

In the second place, this method of calculating being entirely different from that of persons who can see, they cannot understand the particulars of it; this is a considerable inconvenience, because it destroys all connection between the blind and those who operate by sight.

required a great space, and is not legible to others.

The method which appears to me the best, consists in employing characters representing figures, and arranging them nearly as they are composed in printing.

It is better to compose as printers do,

* Melanges Physico Mathematiques.

ALGEBRA.

ALGEBRA.

and in algebra likewise.

The beſt method of writing algebra appears to me, like that of arithmetic, to conſiſt in compoſing in the manner of printers, with this difference, that the characters are a little larger, and muſt be read from left to right. This proceſs has the important advantage of enabling readers by ſight to peruſe and examine the calculations.

GEOGRAPHY.

Eaſy method of forming tangible maps;

There is but one method of uſing geographical maps and the globes: it is ſimple and eaſy. A ſmall ſtring or thread is paſſed on all the windings or outlines of the map. The rivers and mountains are diſtinguiſhed by different threads. Grains or prominences of different ſizes and ſhapes are paſted on the towns and other remarkable places, &c. With one of theſe maps others may be procured by the preſs, in which the outlines will be raiſed into relief.

and multiplying copies.

MUSIC AND BOOKS.

Muſic books.

Citizen Haüy has compoſed muſic books for the uſe of the eſtabliſhment of the *Quinze Vingts* at Paris, the characters of which project, or are emboſſed: this method is without doubt uſeful, but it is ſcarcely applicable but to great foundations.

WRITING.

Writing on paper with a blunt ſtile is legible by the protuberance.

Citizen Haüy's method conſiſts in uſing an iron pen, the point of which is not ſplit; by writing without ink and preſſing on a ſtrong paper, the blind man produces a character in relief, which he can immediately read by paſſing his fingers over the projecting characters on the oppoſite ſide of the paper, in the contrary direction. This relief is ſufficient, provided a ſoft ſurface be placed under the paper, ſuch as leather, blotting paper, &c.

Frame for writing ſtraight.

In order to write in a ſtraight line, a frame is put on the paper, having a number of braſs wires acroſs, parallel to the direction of the writing, at the diſtance of about three quarters of an inch.

His method of writing in relief has this advantage, that the blind can read what they have written, after a little practice; but

but it has the inconvenience of preventing perſons that can ſee from reading it, unleſs they have alſo practiſed the ſame method *.

How to make the writing *viſible.*

A black writing may likewiſe be obtained with this iron pen, by placing on the writing paper another ſheet done over with hogs-lard and ivory black, covered again by another, through which the letters are traced. The inconvenience of this method is, that the blind cannot read what they have written.

Inſtead of the frame with parallel threads, I employ a board which has ſeveral conveniences.

A more convenient frame for ſtrait lines.

This board is about 10 inches in breadth, and 16 high; on the left ſide, and near half an inch from the edge, there is a ſmall dove-tailed groove from top to bottom. A rule three quarters of an inch wide, and 4½ long, ſlides in this groove by a piece ſo formed as to fit the groove. Another rule five eighths of an inch wide, is fixed perpendicularly to the firſt rule in the form of a T, and ſerves very conveniently to direct the little finger of the hand that writes. Laſtly, the left ſide of the board is divided into notches almoſt half an inch diſtant from each other: this being done, the following is the method of writing:

Method of writing with ink.

The paper is placed under the long rule, and is faſtened by two ſcrews eaſy to be imagined, which preſs it againſt the board: the writing is performed by keeping the hand lightly over the rule, the little finger being behind. The rule is ſhifted from line to line very readily by the left hand, the thumb being applied to feel the notches. At the finiſh of every line ink is dipped or taken (in a ſilver pen formed in the uſual method) out of a broad flat veſſel conſtantly containing ink to the depth of between one eighth and one quarter of an inch.

By this proceſs, the lines may be written as near each other as may be required. The rule directs the hand better than a wire, which is too flexible, and not broad enough. When, after diſcontinuing writing the writer is deſirous of proceeding, the rule indicates the point to begin from, &c.

* If the paper be rubbed with pumice, or have ſome earthy powder in the ſize, and a blunt point of hard pewter be uſed, the letters will be neat and viſible.---N.

INK IN RELIEF.

Desideratum; a visible and tangible ink.

It would be a very useful invention, if an ink could be formed which in coming from the pen should leave a mark that would project. This ink would have the double advantage of enabling the blind and those who see to read at the same time writing, the figures of arithmetic, the characters of algebra, and the notes of music. I have not yet thought of any satisfactory expedient for this purpose, and shall not therefore enter into any further detail.

GEOMETRY.

Saunderson's method of forming geometrical figures with pins and threads.
A better method; by wires on a flat cushion.

Saunderson, in order to construct geometrial figures, placed pins in holes made in a board, and passed threads from one pin to another. My method appears to be preferable in every respect; it is this: with four sides of wood I have constructed a frame, the length of which is about 20 inches, the breadth 16, and thickness three quarters of an inch. I have covered the two faces with woollen cloth rather open and soft, and have filled the intermediate space with other loose and soft woollen cloth, so as to form a flat cushion three quarters of an inch thick.

I have very small iron wires, bended at right angles at the two ends, which are pointed and polished. These iron wires are easily fixed in the cushion, and form lines in relief, which nothing can derange. I have an assortment of all lengths, as well as circles, semi-circles, parabolas, ellipses, &c. Lastly, I have letters in relief, with a point in order to fix them on the cushion.

This method is very ready and comprehensive.

By means of this little apparatus, there is no figure in geometry, however complicated, but may very speedily be represented in relief.

We might likewise, and with advantage, use this cushion for making figures with pins and threads; but this method, though better than that of Saunderson, is not so good as the iron wires here described. Experience is decisive in this respect.

and is applicable to music, &c.

It is easy to perceive that the above method is likewise the best for the notes of music; but the limits I have adopted will not allow me to enter into further details.

XI. *Observations*

XI.

Obfervations on the real Nature of the Precipitates formed by the Pruffiates in Acid Solutions of Barites, and the Affinities of the Pruffic Acid *. *By* CITIZEN GUYTON.

BERGMAN announced, that the acid folutions of barites are precipitated by the faturated Pruffian alkali. This fact, confirmed by the obfervations of a great number of chemifts, appeared to many as a new indication of its metallic nature, already fufpected on account of its great weight. Lavoifier in particular thought it very probable, that the difficulty of reducing it to the metallic ftate proceeded from its having more affinity with oxigen than carbon †.

Precipitation of barites by Pruffian alkali confidered as an indication of its metallic nature.

The little fuccefs of the attempts to effect this reduction having in fome meafure caufed the notion of this compofition to be abandoned, the attention was again fixed by the phenomenon of the precipitation of an earth by a re-agent, which according to the generally received opinion, afforded a characteriftic indication of the prefence of a metal. It was then fufpected to be owing to fome accidental caufe. M. Meyer of Stetin, announced in 1786, in Crell's Annals, that the prnffiate of potafh, if very pure and carefully prepared, did not precipitate barites from its folutions. Kirwan and Klaproth adopted this opinion from their own experiments. Moft chemifts however continued to believe that the precipitation took place, whatever might be the means of purification of the proof liquor. Pelletier, amongft others, fuppofed it one of the moft effential diftinctive characters of barites and ftrontian ‡.

This notion abandoned,

and the precipitation by pure pruffiate denied.

Some time ago being defirous to try a pruffiate of lime recently prepared with lime of marble, and very clear, I poured into it a folution of carbonate of potafh. The liquor inftantly became milky and opake. As it is certain that no metallic fubftance was prefent, I could only attribute this decompofi-

Carbonate of potafh precipitates pruffiate of lime,

* Read at the fitting of the clafs of Philofophical and Mathematical Sciences of the Inftitute, the 2d Thermidor in the year 10, and inferted in the Annales de Chimie, XLIII. 185.

† Traité Elementaire, &c. Vol. I. p. 174.

‡ Mem. de Chimie, Vol. II. p. 454.

By double affinity.

tion to a change of base of the two salts by double affinity. I then purposed to examine whether this might not be absolutely the same phenomenon which had given rise to the suspicion of the metallic nature of barites.

Mr. Henry's experiments from the Philos. Journal.

I was anticipated in these researches by Mr. W. Henry. He has succeeded in placing this point of theory in the clearest light: I shall repeat in a few words the experiments that led him to it, and which he published in Mr. Nicholson's Journal *; after which I shall make some reflections upon one of the consequences he deduced from them, which appears to me to require amendment.

Prussiate of barites formed,

Mr. Henry's first object was the direct composition of prussiate of barites, in order to obtain a perfectly pure prussiate of potash: the following is his process, which he affirms that he carried into effect, and was suggested to him by observing the decomposition of the sulphate and carbonate of potash by the prussiate of barites.

by calcining the carbonate of barites; dissolving the earth in water, and add. Prussian blue till it is no longer discoloured by the earth seizing the p. acid.

He calcines the carbonate of barites; he dissolves this earth in boiling water, and adds Prussian blue till it is no longer discoloured. The filtered liquor sometimes becomes turbid by cooling, and deposes a little oxide of iron. He filters it again, and at the end of several hours small yellowish crystals are formed, which are the prussiate of barites. He obtains an additional quantity by evaporation.

The crystallized prussiate of barites is decomposed by carbonate of potash, and thus gives fine crystals of prussiate of potash.

This prussiate of barites in powder is thrown into a hot solution of carbonate of potash, until it no longer restores the colour of blue paper. The author observes, that it is better to employ more prussiate than is necessary for the decomposition of the carbonate. He digests the mixture for about an hour; then filters the liquor, evaporates it slowly, and obtains by this method very fine crystals of prussiate of potash.

Last purification by acetous acid.

These crystals nevertheless sometimes contain as much as 0.24 of oxide of iron; the greatest part of this is separated by digesting the liquor before filtrated, with a little acetic acid, which has the advantage of forming with the potash an uncrystallizable salt, and consequently cannot mix with the crystals of the prussiate.

The precipitated carbonate of barites may again be used in the same operation.

* Vol. IV. Quarto, p. 30, 170.

Let

Let us now obſerve the uſe Mr. Henry made of the pruſſiate of potaſh thus prepared to reſolve the controverted queſtion.

He poured ſome of it into a ſolution of muriate of barites. No precipitation followed; the mixture was not in the leaſt diſturbed, and he was inclined to think with Meyer and Klaproth that a pure pruſſiate does not precipitate barites.

The pure pruſſiate of potaſh did not precipitate the muriate of barites at firſt;

But having obſerved the mixture half an hour afterwards, he perceived ſmall cryſtals forming on the ſides of the veſſel; and at the end of ſome hours they were conſiderably augmented.

but it did in half an hour.

Theſe cryſtals when examined preſented the following characters:

Character of the precipitated cryſtals.

1. They are very ſparingly ſoluble in cold water; one ounce is required to diſſolve a quarter of a grain *.

2. Warm water diſſolves them rather quicker, though in a very ſmall proportion.

3. Theſe ſolutions afford with ſulphuric acid and ſulphate of potaſh a precipitate of ſulphate of barites; and Pruſſian blue with ſulphate of iron.

They were undoubtedly pruſſiate of barites.

4. Theſe cryſtals are completely diſſolved in muriatic acid diluted with water, and this ſolution afforded unequivocal ſigns of the preſence of pruſſiate of barites.

5. Theſe cryſtals heated to redneſs in a ſilver crucible became black, and loſt their form; muriatic acid poured on the coaly reſidue produced an effervefcence, and muriate of barites was formed.

Mr. Henry draws ſeveral conſequences from theſe facts. The *firſt*, that he was deceived, as well as thoſe who announced, that pure barites does not decompoſe pruſſiate of potaſh.

Inductions from theſe facts.

The *ſecond*, that barites ought to be placed before potaſh in the column of affinities of pruſſic acid.

The *third*, that the precipitation effected by pruſſiate of potaſh in a ſolution of muriate of barites, is the product of a double affinity.

The *fourth*, that *barites differs in this reſpect from the other earths, and approaches the metals.*

Of theſe four propoſitions, the three firſt ſeem to me to be open to no objection; for though Mr. Henry does not relate

* As the Engliſh ounce is here ſpoken of, which contains 480 grains, cold water only diſſolves about 0,0005 of its weight.---G.

any

any direct experiment on this subject, it is known, and I have myself observed it, that the aqueous solution of barites poured into a solution of alcaline prussiate, decomposes it by simple affinity.

Objection to the inference that barites approaches to the metals:

It is not the same with the last conclusion of Mr. Henry; for though these acid solutions of barites are decomposed by the alcaline prussiates, precisely like the metallic solutions by the way of double affinity, or the concurrence of divellent forces which determine the exchange of bases, it in no respect follows that barites is on that account removed from the earths, or approached to the metals. If that were the case, the same must be asserted of all substances, which present the same phenomenon, and it would no doubt be a matter of surprize to find this reasoning lead us to assimilate to the metals *lime, strontian, magnesia, potash, soda,* and even *ammonia,* since the Prussic acid can take all these bases from their solvents when the sum of the divellent affinities is in its favour.

because the precipitation is by double affinity, and happens with other earths and alkalies,

namely potash;

I have proved this in the case of *potash* by the experiment already related, where it is seen that it takes the Prussic acid from the lime, and yields to it the carbonic acid.

soda;

This effect is still speedier with carbonate of *soda,* and prussiate of lime.

strontian;

The solution of nitrate of *strontian,* is likewise decomposed by the prussiate of lime.

lime;

If a few drops of prussiate of lime be poured into a solution of sulphate of ammonia in five or six minutes, a whitish frothy film of the thickness of about a quarter of an inch thick swims at the surface, and is at last deposed in small flakes.

magnesia;

Lastly, sulphate of magnesia mixed with prussiate of lime becomes clouded, and a precipitate is formed.

and because the order of affinity is not the only distinctive character of bodies.

Let us therefore return to the true cause of these phenomena, the action of the double affinities in which we still trace the simple elective attractions, the order of which is totally independent of that series, or arrangement in which we place such substances as have several other properties in common. Therefore the station which barites holds before potash in the columns of the attractions of prussic acid, ought no more to be applied in its classification, than the position of lime before the fixed alcalis in the columns of the oxalic, tartarous, and boracic acids, &c. With much more foundation

 it

it has been propofed to rank barites with the alcalis, whofe chemical action it in fact performs in many circumftances, but from which it however differs, as I have elfewhere obferved *, in fome characters, which may be confidered as limits, and particularly by its infolubility in alcohol.

XII.

A Review of fome Experiments, which have been fuppofed to difprove the Materiality of Heat. By WILLIAM HENRY. *From the Memoirs of the Literary and Philofophical Society of Manchefter, Vol. V. Part II.*

THE following remarks, on the fubject of heat, were written foon after the publication of Count Rumford's Inquiry concerning the Source of the Heat evolved by Friction; and of the interefting effays of Mr. Davy, which appeared in Dr. Beddoes's Weft Country Contributions. They were tranfmitted to Dr. Beddoes, for publication, about the clofe of the year 1799; but circumftances, with which I am unacquainted, have, I believe, induced the Doctor to decline the continuation of his periodical work. Thefe circumftances I deem it neceffary to ftate; becaufe, had the effay been written nearer the period of its publication, it would probably have affumed a very different form. At prefent, I have not leifure to review the fubject, or to attempt any material alteration; and ftill lefs, to examine whether I have been anticipated by any of the authors whofe effays have been publifhed during the two laft years. Preface.

A Review of fome Experiments, which have been fuppofed to difprove the Materiality of Heat.

It has long been a queftion among philofophers, whether the fenfation of heat, and the clafs of phenomena arifing from the fame caufe, be produced by a peculiar kind of matter, or by motion of the particles of bodies in general. The former of thefe opinions, though far from being univerfally admitted, is now moft generally received; and the peculiar body, to which the phenomena of heat are referred, has been demonftrated by M. Lavoifier, caloric. Againft the doctrine of the French Queftion whether heat be matter or motion.

* Annales de Chimie, Vol. XXXI. p. 267.

fchool

school some forcible arguments have lately been advanced by Count Rumford and by Mr. Davy, both of whom have adopted that theory respecting heat, which assigns, as its cause, a motion among the particles of bodies.

Mr. Davy's opposition to the matter of heat is by *reductio ad absurdum.*

The method of reasoning, employed by Mr. Davy, in proving the immateriality of the cause of heat, is the *reductio ad absurdum*, i. e. the oppugned theory is assumed as true, together with its applications; and facts are adduced, directly contradictory of the assumed principles. I shall take the liberty of offering a statement of the argument, rather different from that of Mr. Davy; though I trust without misrepresentation, or any material omission.

Another statement: Assuming the existence of caloric, &c.

Let heat be considered as matter; and let it be granted, that the temperature of bodies depends on the presence of uncombined caloric. Now, if the temperature of a body be increased, the free caloric, occasioning that elevation, must proceed from one of two sources; either, 1*stly*, It may be communicated by surrounding substances; or, 2*dly*, It may proceed from an internal source, *i. e.* from a disengagement of what before existed in the body, latent or combined. But the temperature of bodies is uniformly increased by friction and percussion, and, necessarily, in one of the foregoing modes.

Heat by friction in vacuo,

I. Mr. Davy found, by experiment, that a thin metallic plate was heated by friction in the exhausted receiver of an air-pump, even when the apparatus was insulated from bodies capable of supplying caloric, by being placed on ice. This experiment he considers as demonstrating, that the evolved caloric could not be communicated by surrounding bodies.

does not exclude caloric,

To the inference deduced from this experiment it may be objected, that the mode of insulation was by no means perfect. Admitting the vacuum produced by the air-pump to have been complete, still the supply of caloric could not thus be intirely cut off; since it has been shewn by Count Rumford, that caloric passes even through a torricellian vacuum. If, therefore, friction produce in bodies some change, which enables them to attract caloric from surrounding substances, this attraction may be equally efficient in an exhausted receiver, as in one containing an atmosphere of mean density. It would be an interesting subject of experiment; to determine the influence of atmospheres of various densities, as conductors of caloric; for, since effects are proportionate to their causes, and it is ascer-

because a vacuum conducts.

 tained

tained that common air conducts caloric better than it is conveyed through a vacuum, as 1000 is to 702, it may be expected that the ratio will hold in all intermediate degrees.

In Count Rumford's masterly experiment, the metal, submitted to friction, was encompassed by water; and air was carefully excluded from the surfaces in motion. Yet the water became hot, and was kept boiling a considerable time. In this case, the only obvious source of caloric, from without, was through the borer employed in producing the friction; if it be true, as the Count has observed, that the water could not, at the same instant, be in the act of giving out and receiving heat. The same objection to the communication of heat, from an external source, exists also in thus explaining Mr. Davy's experiment: but I cannot admit that the argument is demonstrative, in proving the evolved caloric not to be derived from external substances; for no absurdity is implied in supposing, that a body may be receiving caloric in one state, and giving it out in another. We have an example of the simultaneous admission and extrication of a subtle fluid, the materiality of which is admitted by Mr. Davy, in an excited electric, which, at the very same instant, receives the electric fluid from without, and transfers it to the neighbouring conductors. In an ignited body, also, the two processes of absorption and irradiation of light, are, perhaps, taking place at the same moment.

In Count Rumford's experiments (Philos. Jour. 4to, II. 106), caloric might be supposed to be absorbed and emitted at the same time.

Instance in electricity.

II. Another cause of the increase of temperature in bodies, is the liberation of their combined caloric; and, if this be a source of temperature, the absolute quantity of caloric in a body must be diminished by friction. That no such diminution really takes place, we have the evidence of two experiments—the one of Mr. Davy, the other of Count Rumford. Mr. Davy, by rubbing together two pieces of ice, converted them into water. Now water, *ex hypothesi*, contains more caloric than the ice from which it was formed; and, on the same hypothesis, the absolute quantity of caloric in ice is diminished by friction and liquefaction, which is absurd. Count Rumford also ascertained, that the specific heat of iron was not diminished when converted by a borer into turnings, and consequently when it had been the source of much temperature. In explanation of these facts, we may be allowed to assume the communication of caloric from surrounding bodies, till this communication has been demonstrated to be impossible. But even were the

In bodies which emit heat by friction; but do not have their capacity diminished;

it may be supposed the heat came from without;

the impoffibility eftablifhed, it would yet remain to be proved, that the evolved caloric does not proceed from an internal fource; and this can only be done by an accurate comparifon of the quantity of caloric in bodies, before and after friction. Now, in inftituting this comparifon, it is implied, that we poffefs means of determining the abfolute quantity of caloric in bodies, and that we can compare quantities of caloric with as much certainty as we can obtain from an appreciation by weight or by meafure. Such perfection however does not, I apprehend, belong to the prefent ftate of our knowledge refpecting heat; for I have always been diftruftful of that part of the doctrine, which affigns the ratio of heat latent in bodies. The grounds of this diftruft I fhall ftate pretty fully; for, if it can be proved that we have no accurate conceptions of quantity, as appertaining to heat, all arguments againft its materiality, derived from fuppofed determinations of its quantity, muft be inconclufive.

and the experiments for deciding capacities are queftionable.

The only clear conceptions which the mind has of quantity, are derived either from a comparifon of the magnitude, or of the gravity of bodies. In the inftance of caloric, both thefe modes of menfuration fail us. We cannot eftimate the bulk of a fubftance which eludes our grafp and our vifion; nor have we yet fucceeded in comparing its gravity with that of the groffer kinds of matter, which it furpaffes in tenuity beyond all comparifon. Our notions of the quantity of caloric are derived, not from fuch fimple judgments, but from complicated proceffes of reafoning, in the fteps of which, errors, fatal to the whole, may perhaps fometime appear.

It is afferted that our only clear conceptions of quantity are from magnitude or weight; neither of which can be exhibited in caloric.

Whatever be the nature of caloric, whether it be a body *fui generis*, or a quality of other bodies, its effects are peculiar and appropriate; and, like all other facts, bear a proportion to the energy of their caufe. Expanfion, for example, it is proved by experiment, keeps pace with the actual increments of heat; and on this principle is founded the thermometer, the great agent in the acquirement of all our ideas refpecting heat, both abfolute and relative. The competency of this inftrument, however, to afford information of the quantity of caloric, is limited by the following circumftances:

Caloric is peculiar in its increments being denoted by expanfions.

1ft, The mercury of the thermometer indicates only the quantity of heat which it has itfelf acquired, and by no means that contained in furrounding bodies. *2dly*, The fcale of expanfion

The thermometer indicates no abfolute quantities,

panfion is wholly arbitrary, commencing far from the abfolute privation of heat, and falling far fhort of its maximum. *3dly*, The caloric, latent in bodies, or chemically combined with them, has no effect on the thermometer. *4thly*, The experiments of Dr. Crawford, though fufficient to fhew that the expanfion of the mercury of the thermometer bears a ratio to the actual increments of heat, in any temperature between the boiling and freezing points of water, by no means prove that this proportion holds univerfally.

and the laws developed by Crawford apply only to a fmall part of this fcale.

Equal weights of heterogeneous bodies, it is prefumed, contain unequal quantities of caloric; and the ratio of thefe quantities is approximated in the following manner.

Equal weights of the fame body, at different temperatures, give, on admixture, the arithmetical mean: but equal weights of different bodies, at different temperatures, afford a temperature which varies confiderably from the mean. Thus a pound of water at 100 degrees, and a pound at 200°, give the temperature of 150°; but a pound of water at 200°, and a pound of mercury at 100°, afford, not the mean, but a temperature confiderably higher. Hence it follows that a pound of mercury has not the power of fixing and retaining fo much caloric as a pound of water: and the fixation of more heat, by the water than by the mercury, is afcribed to the fuperior energy of a power inherent in both, and termed capacity for caloric.

Common method of finding the ratios of caloric by the common temperature + or — than the mean.

From an extenfive feries of experiments Dr. Crawford infers, that the capacities of bodies are permanent, fo long as they retain their form. Thus, the capacity of water has to that of mercury the ratio of 28 to 1, at any temperature between 32° and 212°. The difference of capacities of bodies, it is inferred, therefore would continue the fame, down to the abfolute privation of temperature. Imagine, then, two bodies at this point of privation: they may ftill contain unequal quantities of combined caloric; for, when chemically combined, caloric does not produce temperature. On Dr. Crawford's hypothefis, thefe comparative quantities of combined caloric, in the two bodies, may be learned by obferving the ratio of temperature, produced, by the addition to each, of fimilar quantities of heat. This fuppofition, however, is manifeftly gratuitous; and the contrary might be maintained with equal or greater probability: for it may be fuppofed that, at this affumed negation of temperature, one body renders

Capacities permanent by Crawford's hypothefis,

not applicable at remote and unknown temperatures,

latent

latent more caloric than another, because it actually contains less; as certain dry salts attract more water from the atmosphere than others containing much water of crystallization.

not probable. The commonly employed mode of ascertaining the specific caloric of bodies, is founded, therefore, on an assumption, which is deficient in the character of a datum, and which itself requires proof.

Theorem for finding the absolute zero, fallacious.

If these objections be valid, they will apply also to shew the fallacy of the theorem, for finding the absolute zero of bodies. By this term some philosophers appear to understand the point of absolute privation of caloric, both free and combined. I apprehend, however, that in strict propriety it can only be used to signify the negation of *uncombined* caloric, or, as Dr. Crawford expresses himself, the point of absolute cold. As applied, however, to water, it is evident that the whole quantity of

The method.

heat is understood. In ascertaining the zero, say these calculators, the capacity of ice to that of water is as 9 to 10. It is plain, therefore, that when water freezes, it must give out $\frac{1}{10}$th of its whole heat, and this 10th part is found to answer to 146° of Fahrenheit. Consequently its whole heat is 10 times 146, or 1460°; and hence the natural zero is 1460 — 32 or 1428°. Now of this estimate it is a datum, that the capacities of ice and water have precisely the above ratio. But if the general formula, for ascertaining the specific caloric of bodies, be founded on erroneous principles, it cannot serve as the groundwork of any solid conclusions.

Arguments including these doctrines, therefore prove nothing either way.

The materiality of caloric may, I apprehend, be maintained, without admitting that we have made any steps towards determining its quantity in bodies; and the arguments of Count Rumford and Mr. Davy are not demonstrative, because they assume, that this part of the doctrine of caloric cannot be relinquished, without abandoning it *in toto*. I may be permitted, therefore, to state my reasons for believing caloric to be matter; which would have been unnecessary, had the contrary been proved, with all the force of mathematical demonstration.

That caloric is matter;

Avoiding all metaphysical reasoning on the nature of matter, and assuming the generally received definition, as sufficiently characterizing it, I shall examine how far this general character of matter applies to the individual—caloric. Caloric oc-

because it expands bodies, and therefore occupies space, and is impenetrable:

cupies space or is extended, because it enlarges the dimensions of other bodies; and, for the same reason, it is impenetrable, since

ſince if it could exiſt, at the ſame time, in the ſame place, with other bodies, their volume would never be enlarged by the addition of heat. Of form or figure, as only a mode of extenſion, it is unneceſſary to prove that caloric is poſſeſſed; and indeed there is perhaps only one general quality of matter, that will not be allowed it, viz. attraction. That caloric is influenced by the attraction of gravitation, or by coheſive attraction, has never yet been proved. Yet the various experiments of Buffon, Whitehurſt, Fordyce, Pictet, &c. cannot be alledged as proofs, that it is actually devoid of this property; ſince they only decide, that the ſmall quantities, which can be artificially collected, are not to be ſet in the balance againſt the groſſer kinds of matter. One kind of attraction, *that* which has lately been termed chemical affinity, may, I think, after a full ſurvey of phenomena, be fairly predicated of caloric—and if its poſſeſſion of this quality be rendered probable, we ſhall thence derive a powerful argument in favour of its materiality.

It has not been proved deſtitute of weight.

That chemical affinity has a conſiderable ſhare in producing the phenomena of heat, appears probable from the following conſiderations:

It appears to have chemical attraction;

1. All the characters, diſtinguiſhing caloric when ſeparate, ceaſe to be apparent, when it has contributed to a change of form in other bodies; and the properties of the ſubſtances ſo changed are alſo materially altered. Now this is the only unequivocal mark of chemical union that we can apply in any inſtance; and chemical union implies the exiſtence and efficiency of chemical affinity.

becauſe its properties appear in combination;

2. The relation of caloric to different ſubſtances, appears to obſerve that peculiar law, which, in other inſtances, is termed elective affinity. If a compound of two or more principles, a metallic oxide for inſtance, be expoſed in a high temperature, the caloric forms a perfect union with the one, but not with the other. In certain inſtances, caloric is evolved when two ſubſtances, attracting each other more powerfully than they attract caloric, produce, on admixture, an elevation of temperature. In other inſtances, caloric is abſorbed when it is attracted by the new compound more ſtrongly than by the ſeparate components. Such facts warrant the deduction, that caloric is ſubject to the laws of chemical affinity. But the preciſe order of its affinities remains to be decided by future experiments.

and it ſhews elective affinity; ſingle:

3. Caloric

and double or compound;

3. Caloric seems also, on some occasions, to bear a part in the operation of double elective affinities. In this way it produces decompositions, which, by single affinity, it is incapable of effecting. Thus a most intense fire does not expel intirely the carbonic acid from alkalis. But when the affinity of an acid for an alkali concurs with that of carbonic acid for caloric, a decomposition ensues.—Again, water may be submitted to the highest temperature, without imparting a gaseous form to the hidrogen which it contains; but the conspiring affinity of a metal for oxigen occasions the production of hidrogenous gas. On this principle, many chemical facts are resolved into the law of *double affinity*, which are, at present, explained by that of single elective attraction.

and by intermedium.

4. Caloric acts, sometimes, as an intermedium in combining bodies, which, without its aid, are not susceptible of combination. Thus carbon and oxigen do not evince any tendency to combination, at the ordinary temperature of the atmosphere; but caloric brings them into union, and constitutes, itself, part of the resulting compound. This, and a variety of other instances, have a striking resemblance to what is called *intermediate affinity*.

Capacity and affinity are not synonimous terms.

In the theory of Dr. Crawford, no influence is allowed to chemical affinity over the phenomena of heat; and indeed *that* philosopher expresses a decided opinion, that elementary heat is not capable of uniting chemically with bodies. Hence it appears, that the difference between the terms affinity and capacity is not merely a verbal one; but that they are actually expressive of different powers or causes: and the question, therefore, which of these terms shall be adopted, in the description of facts, is one involving the determination of causes.

On the word capacity as applied by Crawford.

The term capacity for heat is employed, by Dr. Crawford and others, to denote, in the abstract, that power, by which different kinds of matter acquire different quantities of caloric. But in the various applications, that are made of this theory, a more precise meaning is often affixed to it; and the term is applied, in much the same sense, which it has in common language. When thus understood, a difference of capacity necessarily implies a difference in the extent of the spaces, between the minute particles of bodies; and that these differences occasion the varieties, observed in the acquirement of heat.

heat by different bodies. On this theory, there is no active principle or power inherent in bodies, and more active in some than in others,—no tendency in the matter of heat to attach itself, in preference to any one substance. The assigned cause of the phenomena of heat is not, I apprehend, adequate to produce the effects ascribed to it.

It usually implies no power but mere locality.

On the theory of capacities, a change of form is, in certain instances, antecedent to the absorption of caloric. Thus, when ether is converted into gas, on removing the pressure of the atmosphere, according to this hypothesis, the capacity of the ether is increased by its volatilization; and the change of form is prior to, and the cause of, the absorption of caloric. The order of events, then, in the volatilization of ether, is first an alteration of form; next a change of capacity; and lastly an absorption of caloric. On this hypothesis, ether may exist in the state of gas, without containing a greater absolute quantity of caloric, than in a liquid form. But such an interpretation of phenomena is directly contradictory to an established principle, admitted, even by those who prefer the doctrine of capacities, viz. that all bodies, *during* their conversion from a fluid to a vaporous state, absorb caloric. It is at variance, also, with observed facts: for if a thermometer be immersed in a portion of ether, confined under the receiver of an air pump, the temperature of the ether will be found to sink gradually, during the exhaustion of the air; and the evaporation becomes proportionally slower, till, at last, it is scarcely perceptible. We may, therefore, infer, that at a certain point of diminished temperature, the volatilization of ether would entirely cease, if the supply of caloric, from surrounding bodies, could be completely intercepted. But on the theory of capacities, the evaporation should proceed as rapidly at the close, as at the commencement, of the process —or, in other words, evaporation should be wholly independent of temperature, which every one knows is contrary to fact.

By this theory the change of capacity may precede the transition of heat.

It may be considered therefore, as extremely probable, that the tendency of ether to assume a gaseous form depends on its chemical affinity for caloric. But, (it may be asked) how is this affinity counteracted by an increased pressure, and augmented by a diminished one?

Whence it is most probable that the effects arise from affinity.

A cir-

Mechanical pressure prevents the formation of gas; but is not an argument against this doctrine.

A circumstance, absolutely essential to the formation of gases, is, that free space shall be allowed for their expansion. Mechanical pressure acts as a counteracting force to this expansion; and either prevents it completely, or partially, according to the degree of its application. But from this fact, no argument can be drawn against the existence of chemical affinity, as an attribute of caloric. Two opposite forces in physics may be so balanced, that neither shall produce its appropriate effect. Thus a body, impelled in contrary directions, may remain at rest, yet the operation of the opposing forces, in this case, cannot be denied. Even in chemistry, we have unequivocal examples, in which the action of the affinities is suppressed by more powerful causes. Thus bodies, that have a strong chemical affinity, are kept perfectly distinct, even when placed in contact, by the affinity of aggregation. The only inference, then, that can fairly be adduced from the effects of pressure, in preventing the formation of gases, is, that it is a power, sometimes superior, in energy, to that of chemical affinity.

Caloric having all the properties of matter, but gravity may be taken to be matter.

Since, therefore, caloric is characterized by all the properties, except gravity, that enter into the definition of matter, we may venture to consider it as a distinct and peculiar body. Nor is its deficiency of gravity sufficient to exclude it from the class of material substances. Such nicety of arrangement might, with equal propriety, lead us to deny the materiality of light, the gravity of which has never yet been proved: for, besides the experiments of Mr. Michell, which failed in ascertaining this property of light, we have several chemical facts tending to the same conclusion. Thus Mr. Cavendish, after firing a mixture of hidrogenous and oxigenous gases, in a close vessel, a process during which much light is always emitted, found not the smallest diminution of weight.

Phenomena of heat differ from those of motion.

To have completed this defence of the material nature of heat, it would have been proper to have pointed out the circumstances, in which the phenomena of heat differ from the known and acknowledged phenomena of motion. At present, however, I have not leisure to pursue the subject at much length; and, though several points of disagreement would doubtless be found, I shall mention only one of the most marked and decisive.

Motion

Motion is an attribute of matter, independently of which it cannot possibly subsist. If therefore, the phenomena of heat can be shewn to take place, where matter is not present, we shall derive, from the fact, a conclusive argument against that theory of heat, which assigns motion as its cause. Now, in the experiment of Count Rumford, before alluded to, heat passed through a torricellian vacuum, in which, it need hardly be observed, nothing could be present to transport or propagate motion This experiment, in my opinion, decidedly proves, that heat can subsist independently of other matter, and consequently of motion—in other words *that heat is a distinct and peculiar body.*

Heat can pass through a vacuum; but motion surely cannot be transmitted where there is no matter. Hence the heat is matter.

XIII.

Enquiries concerning the Dilatation of the Gases and Vapors. Read to the National Institute of France. By CIT. GAY LUSSAC *.

ART. I. *The Object of this Memoir.*

NATURAL philosophers have been long engaged in researches on the dilatation of the gases; but their labours present such variations in their results, that so far from having fixed the general opinion, they have on the contrary left us great reason to wish for more accurate investigations.

The researches on the gases are inaccurate.

The attention of philosophers has not been equally directed to the dilatation of vapors. Though the prodigious effects of the vapor of water has long been known and applied with the happiest effect, Ziegler and Bettancourt are the only persons who within my knowledge have attempted to measure them. Their experiments however are not adequate to shew the true dilatation of the vapor; for as there was always water in their apparatus, every new degree of heat not only produced a dilatation of the vapor formed by the preceding degrees, but also increased the volume by the formation of new vapors; two causes which evidently tend to raise the mercury in their manometer †.

The expansions of vapor have been little examined.

The

* From the Annales de Chimie, XLIII. 187.

† The apparatus of Bettancourt consists in a boiler of copper closed by a cover of the same metal, through which pass three tubes.

The

The thermometer does not indicate the real quantities of heat.

The thermometer, as it is at present conſtructed, cannot be applied to point out the exact proportions of heat, becauſe we are not yet acquainted with the relation between its degrees and the quantities of heat. It is indeed generally thought that equal diviſions of its ſcale repreſent equal tenſions of caloric; but this opinion is not founded on any well decided fact.

Numerous demands in philoſophy and the arts for the knowledge of the expanſion of gas and vapor.

We muſt admit therefore, that we are ſtill far from poſſeſſing poſitive knowledge reſpecting the dilatation of the gaſes and vapors, and the correſpondent march of the thermometer; nevertheleſs, we have daily occaſion in natural philoſophy and chemiſtry to reduce a given volume of gas from one temperature to another, to meaſure the heat diſengaged or abſorbed in the changes of conſtitution of bodies; or that diſengaged or abſorbed by the ſame body in paſſing from one temperature to another; and in the arts to calculate the effects of fire engines, or to eſtimate the expanſions of various bodies; in meteorology to determine the quantity of water held in ſolution by the air, a quantity which varies with its temperature and its denſity, according to a law not yet aſcertained. Laſtly, in the conſtruction of tables of aſtronomical refraction, and in the application of the barometer to the meaſurement of heights, it is indiſpenſible that we ſhould accurately know the temperature of the air, and the law of its dilatations.

The author's inducements to this work.

Though theſe conſiderations were of themſelves ſufficient to render the enquiry into an object of ſuch general utility deſirable, yet the difficulty of the reſearches it demands would have prevented my attempting it, if I had not been ſtrongly engaged by Cit. Berthollet, whoſe pupil I have the honor to be. I am indebted to him for the neceſſary means of executing this work, in which I have often been aſſiſted by his advice,

The firſt ſerves to introduce water, the ſecond admits a thermometer to indicate the temperature of the vapor, and to the third is adapted a recurved barometrical tube, to meaſure the elaſticity of the ſame vapor. A vacuum is made in the boiler by means of an air pump communicating through a tube provided with a ſtop cock.

The apparatus of Ziegler differs little from that of Bettancourt; but as Ziegler did not make a vacuum in his boiler, there is a great difference in their experiments and reſults. See the Hydraulic Architecture of Prony, Vol. II.---G. L.

advice, and that of Cit. Laplace. These great authorities will add to the confidence to which my work may be entitled.

The experiments relate only to the comparative expansion of different gases and vapors at a given temperature.

The experiments which I have undertaken on the law of the dilatation of gases and vapors, and the march of the thermometer not being yet completed, the only object of this memoir will be to examine the dilatation of gases and vapors at a determined elevation of temperature, and to shew that it is the same in all these fluids. But before I relate my experiments, I think it proper to give a short account of what has been done on this subject; and as I shall make observations on each of the methods that have been employed, I shall prefix this amount to the examination of one of the principal causes of uncertainty to which this species of experiments is liable. Although it is very important, and seems to have remained unknown to the generality of philosophers who have examined the dilatations of gases, its influence will be fully admitted when simply pointed out. What I shall say of atmospheric air will apply to the other gases.

Water in the apparatus is the great cause of uncertainty.

This cause of uncertainty is owing to the presence of water in the apparatus. If a few drops of this fluid be left in a receiver full of air, and its temperature then raised to that of boiling water; this water when converted into vapor, will occupy a volume about 1800 times greater than its primitive bulk, and will drive off by this means a very considerable portion of the air contained in the receiver. It necessarily happens then, that when these vapors are condensed, and come to occupy a volume 1800 times less, a dilatation much too great will be attributed to the air remaining in the receiver; because it will be supposed that it was this air which at the temperature of boiling water occupied all the capacity of the receiver. If the temperature be not raised to this degree, the same cause of uncertainty will not the less exist, and its intensity will be relative to the height of temperature: for in this case the water will not entirely evaporate, but the air will dissolve more of it in proportion as its temperature is raised, and it will consequently receive a greater or less augmentation of volume, independent of that occasioned by heat; so that as it passes to a lower temperature, the volume of air which occupied the whole receiver will diminish for two rea-

Explanation of this effect.

ſons, 1ſt. by the loſs of its caloric; 2d. by that of the water held in ſolution. Too much dilatation will be here alſo attributed to the air.

Other liquids, or even ſolids may affect the reſult.

In general, whenever liquids, or even ſolids, as for example, muriate of ammonia are incloſed in gaſes capable of diſſolving them, or of evaporating at the temperature to which they may be expoſed, errors in the determination of the gaſeous dilatations muſt neceſſarily reſult.

Art. II. *Short Account of what has been aſcertained concerning the Dilatation of Gaſes.*

Hiſtory of the ſcience of the expanſions of gaſes, &c. Amontons.

The dilatation of atmoſpheric air by heat was known before the time of Amontons; but it appears that this philoſopher was the firſt who wiſhed to know the extent for a given elevation of temperature. For this purpoſe he encloſed air in a ball ſoldered to the extremity of one of the branches of a reverſed ſyphon, and he plunged this apparatus into a bath of hot water *. The air dilated by the heat compreſſed the mercury, and elevated it into the other branch of the ſyphon; ſo that he judged of the ſpring of the air by the height of the mercury above the level of the ball.

From the various experiments made on unequal volumes of air he concludes: (*Mem. de L'Acad.* 1699, 1702.)

Amontons's reſults on common air

1ſt. "That the heat of boiling water has limits, beyond which it does not paſs.

2d. "That unequal volumes of air equally augment the force of their ſpring by equal degrees of heat, and on the contrary.

3d. "That the heat of boiling water does not augment the force of the ſpring of air beyond the power of ſuſtaining about ten inches in height of mercury."

mechanically compreſſed.

He then proves, that however compreſſed a volume of air may be, the heat of boiling water always augments the force of its ſpring about one third; that is to ſay, that a volume of air compreſſed, for example, by a column of mercury of 60 inches, will ſuſtain (including the weight of the atmoſphere), at the temperature of boiling water, a column of mercury of

* The air encloſed in the ball, not being able to eſcape when mercury is poured in, is found a little leſs in volume than it naturally is; but if no other preſſure is deſired than that of the atmoſphere, it will be very eaſy to avoid this ſlight inconvenience.

about

about 80 inches. He concludes from this, "That the fame degree of heat, however fmall it may be, can always more and more augment the fpring of air, if this air bears a weight more and more great."

Much force from fmall increafe of temperature in condenfed air.

If Amontons had begun to reckon from a degree of heat better determined than that which he called *temperate*, which was then hardly poffible, there might have been deduced from his experiments a fufficiently exact dilatation of atmofpheric air; neverthelefs, as he made them comparatively on volumes of air of very unequal denfity, it may be concluded that *however denfe a volume of air may be, the augmentation of fpring it receives from the fame degree of heat, is always relative to that it poffeffed before the experiment.*

Amontons had not a comparable thermometer.

Nuguet, in endeavouring to verify the refults of Amontons, found others very different. In one of his experiments, the volume of air dilated by the heat of boiling water, and the primitive volume were as 2 to 1, and in two others as 16 to 1. His apparatus confifted of a bottle, which he plunged reverfed into a water bath, and elevated its temperature to that of boiling water. It is eafily feen that this apparatus is extremely defective, as the air was always in contact with the water; and befides, Nuguet had fuffered fome water to remain in the bottle. It is no wonder then that he obtained fuch difproportioned and extravagant refults. (*Mem. de L'Acad.* 1708. Lahire.)

Nuguet's experiments differed from thofe of Amontons,

becaufe his air was in contact with water.

This great difference between the refults of Amontons and Nuguet on the dilatation of atmofpheric air, and the confideration that they had fubmitted it to experiments under uncommon circumftances, engaged Lahire to purfue the fame object. His apparatus was the fame as that of Amontons, excepting that the ball contained a tube, which he clofed after having introduced mercury. By this means, the mercury being on a level with the ball and fyphon, the air on which the experiment was made, was not more compreffed than the furrounding atmofphere. With this apparatus Lahire found, that air dilated from the degree of temperate to that of boiling water, could not fuftain a column of mercury of one third the weight of the atmofphere; he then found in another, the thermometer being lower, and the barometer higher than in the firft experiment, that the air dilated by the heat of boiling water, could not fuftain a column of mercury as great as in the

Lahire's repetition of Amonton's experiments.

the firft. Thefe refults are evidently contradictory; but Lahire not fufpecting any error, concluded that it muft be neceffarily allowed that the nature of air was yet undifcovered.

Lahire objects to the experiments of Nuguet on account of water.

Lahire, in order to give a reafon for the difference between his refults and thofe of Nuguet, a difference much too great not to be owing to fome foreign caufe, remarked, that Nuguet had left fome water in his apparatus; and from hence he judged that it might be this water, as it changed to vapor, and expelled a portion of the air enclofed in the bottle, had prefented fo great a dilatation. He was entirely confirmed in his opinion by the refult of an experiment made in Nuguet's method, a little water being left in the bottle; for he found that the volume of air dilated from the degree of temperate to that of boiling water, and the primitive volume was as 35 and a half to 1. (*Mém. de L'Acad.* 1708.)

Stancari alfo obferved the influence of water.

At the fame time M. Stancari of Bologne proved, that water at an elevated temperature augmented the volume of air. To thefe two philofophers then is owing the knowledge of the influence of water on the dilatation of atmofpheric air; but although they placed it by their experiments in the cleareft light, it has been fince generally mifconceived. The great variations in the refults of philofophers on the dilatation of gafes, may likewife be attributed to the little attention that has been paid to this influence.

The dilatations of air are of confequence in barometrical admeafurements.

It is known that elevations in the atmofphere are given by the logarithms of the correfpondent heights of the barometric column. If the denfity of the air were always the fame, it would be eafy to determine the height of one place above any other known place by barometrical obfervation. Whence it becomes of importance to diftinguifh the caufes that may influence the denfity of the air, in order to make the neceffary corrections in the heights given by the barometer.

Deluc on the barometric meafurement.

Deluc, who has thrown fuch great light on this branch of philofophy, difcovered that heat was one of the caufes. That he might well diftinguifh its effects, he firft fought the temperature at which the logarithms give the heights without correction, and he found by comparing feveral obfervations made in fituations whofe heights he had exactly determined, that this takes place at the temperature of 16° $\frac{3}{4}$ of the thermometer divided into 80 parts, which he calls the fixed temperature. To correct the effects of heat above and below this fixed

Temperature of which the logs. give the height.

fixed point, he compared the heights found by the logarithms with thofe which he had meafured, by attributing to heat the exceffes and deficiences of the firft for the fecond; and he concludes "that near the fixed temperature, the correction for a degree of the thermometer is to the height of the place :: 1 : 21 5" (*Recher. fur les modif. de l'at.* IV. part. ch. III.) Correction for temperature.

Colonel Roy difcovered a much greater dilatation in air. According to him near the 15° of the thermometer divided into 80 parts, the air dilates $\frac{1}{172}$ of its volume for each degree. He likewife found that humid air dilates much more than dry air; but Sauffure obferves, that Colonel Roy having introduced into his manometer either liquid water, or the vapor of water, confounded two things which ought to be feparated, namely, the converfion of water into an elaftic fluid, and the dilatation of air united with fuch vapor. (Phil. Tranfact. 1777, p. 704.) Colonel Roy's allowance greater, but he produced more vapor the greater his heat.

Sauffure fixes the dilatation of air near the 6th degree, at $\frac{1}{235}$ of its volume for each degree. His experiments were made in a great receiver, in which were enclofed a thermometer and barometer, to indicate the variations of the temperature of the air, and the correfpondent elafticity it would acquire. To difcover the influence of water on the dilatation of air he enclofed in his receiver air more or lefs dry, and avoided the production of new vapors. He was fo far from finding this air more dilatable than very dry air, that he on the contrary thought very dry air was a little more dilatable than very moift; but always holding its water in perfect folution. (*Effai fur l'Hygrometrie*, p. 108.) De Sauffure's refult. He avoided the production of new vapor; and in this cafe found dry air rather more dilatable.

Till this period philofophers had confined themfelves to the dilatation of atmofpheric air, and the firft who examined that of the other gafes was the celebrated Prieftley. The following is his procefs: The gafes were not yet examined. Prieftley's experiments.

After having filled a phial of gas over mercury, he adapts to it a bended tube, one of its branches being very inclined, and he leaves a little of the mercury in the neck of the phial, that the expanfion of the gas may make it pafs into the tube. This done, he places his apparatus in a box of wood, joins a thermometer with it, and takes it into apartments of different temperatures. The dilated air caufes the mercury to pafs through a greater or lefs fpace in the tube, and by this fpace meafured The gas was received over mercury, and its expanfion noticed in a recurved tube.

measured in inches, Priestley discovered the dilatations of the different gases. As all these experiments were made with the same phial and tube, which he probably always inclined in the same angle, they gave a comparative result between the dilatations of the different gases, but not the absolute dilatations; because for this purpose he ought to have known the capacity of the part of the tube passed through by the mercury relative to that of the phial, and to have known exactly the inclination of the tube. Priestley says nothing of this. I will no longer then discuss his experiments; particularly as Priestley himself does not place much confidence in them, and wishes them to be repeated in a better manner.

but not with great attention:

Supposing equal volumes of different gases, the expansions measured in inches on the tube would be for 4°, 44 of the thermometer divided into 80 parts.

His results as to the increments only.

	Inches.
Common air - - - -	1,32
Hidrogen gas - - - -	2,05
Nitrous gas - - - -	2,02
Carbonic acid gas - - - -	2,20
Muriatic acid gas - - - -	1,33
Oxigen gas - - - - -	2,21
Azote gas - - - - -	1,65
Sulphureous acid gas - - -	2,37
Fluoric acid gas - - - -	2,83
Ammoniacal gas - - -	4,75

(Exper. and Obser. &c. Book VII. Sect. VI.)

Vandermonde, Monge, and Berthollet on common air and hidrogen.

In a memoir printed among those of the Academy for the year 1786, Citizens Monge, Berthollet, and Vandermonde, concluded from experiment, that for one degree, atmospheric air dilates $\frac{1}{184,83}$ of its volume, and hidrogen gas $\frac{1}{181,02}$.

Guyton and Duvernois.

At length Cit. Guyton considering the variations in the results on the dilatation of atmospheric air, and that there were no direct experiments to determine the dilatation of gases at degrees of heat a little elevated or otherwise, undertook with Cit. Duvernois to examine this object. As their work is the most recent, I shall endeavour for a moment to discover what may have affected their results.

Their

Their apparatus was compofed of a body with a bended tube, by which means, the air expelled by heat from the body, was received in a jar on the pneumato mercurial apparatus. The body filled with the gas on which the experiment was to be made, was plunged into a bath at the temperature of melting ice, and was kept down by an iron cover. The bath was fucceffively heated to the degrees 20, 40, 60, and 80, and they collected in feparate jars the products of the dilations for each of thefe degrees. They then determined the volumes of air expelled from the body by meafuring them in their refpective jars after having reduced them to the temperature of melting ice, and they determined the volume of that remaining in the body.* But fetting afide that their apparatus obliged to afcertain *many data* which muft have been an objection to the exactnefs of their refults, I remark that after the immerfion of the bended tube in the mercury, as they did not introduce new air into the receiver to difplace the mercury which had entered the tube by reafon of the preffure of the mercury in the trough feveral degrees of heat muft be required before any bubble of air can come out; fo that had they reckoned from divifions more nearly together, as from 5° to 5° they would have concluded that from zero; the firft degrees of heat do not caufe the different gafes to undergo any dilatation. Hence they found for the firft twenty degrees a dilatation much too weak for the gafes in general.

Their experiments. The gas was extruded from a body by the heat of a bath, and this excefs received our mercury.

Objection.

The mercury in the tube difturbed their refult.

This caufe of error, tho' great, would not have made the refults of Citizens Guyton and Duvernois fo far from the truth had there not been a ftill greater. I fufpect then that their receiver was not fufficiently dry, and that a little water might have been introduced with the gas. In fact, a decigramme of water would have been fufficient to have influenced their refults very confiderably, particularly in the higher degrees; for, as it changed into elaftic fluid, it would have expelled a great quantity of the air from the receivers.

Their retort or body does not appear to have been dry enough.

The increafing progreffion which they obtained in all the gafes is this way explained, whilft they ought to have obtained a decreafe of volume in the products of each dilatation were reduced to the temperature of melting ice. I fhall obferve that Cit. Guyton expreffes himfelf thus relative to the dilatation of

And from this caufe the expanfions appeared greateft at the higher temperatures.

* Annales de Chimie, VOL. I.

hidrogen

hidrogen gas, (Annales de Chimie, tom, I. page 284.) "The "four products of the dilatation were in this case received in "a veffel which was furrounded with another veffel filled with "ice. Notwithftanding this, the mercury of the trough in- "dicated by the thermometer 2, 3, 4, 6, degrees above zero, "whilft the water of the bath was at the fame time at 20, 40, "60 and 80 degrees, which may have occafioned fome inac- "curacy in the valuation of each of thefe products; but this "cannot be of great confequence, the dilatation being very "little in thefe firft degrees."

Hence it may be inferred that thefe philofophers did not pay more attention to reduce the volumes of the other gafes to zero; and if that was the cafe, this would be another caufe of uncertainty in their experiments.

By comparing the volumes of gafes remaining in the retort with thofe expelled by heat, Cit. Guyton and Duvernois found that oxigen, hidrogenous and carbonic acid gafes and atmof- pheric air underwent a diminution, and they attributed the caufe of it to the combinations which had taken place during the time of performing the experiments. Yet when I ufed very pure mercury, deprived of oxide, I could not obferve any fenfible action between this metal and the gafes from the temperature of melting ice to that of boiling water.

(To be concluded in our next.)

XIV.

Defcription of a new Secret Lock. By J. B. BERARD.

Preliminary Reflections.

Great utility of locks.

SOME machines are fo eminently ufeful as to require no dif- cuffion in that refpect. They are highly interefting to the whole of fociety. Locks are of this number. They are a de- fence fupplementary to the laws, and a remedy for the confe- quences of immorality. From the loweft to the higheft ranks in the ftate, there is no one who does not derive daily advan- tage from this invention. And accordingly different artifts

Secret locks;

* From his Melanges Phyfico Mathematiques. Paris, an 9.

have

have ſucceſsfully employed themſelves upon them, to the production of ſecret locks, exhibiting different degrees of ingenuity.

But it is not ſufficient that this inſtrument ſhould contain a ſecret difficult to be developed by the ſkilful method of concealment. The proprietor of this lock muſt always have, as partakers with him, not only the maker, but alſo his friends, his domeſtics, and generally every perſon who poſſeſſes a lock conſtructed on ſimilar principles. The queſtion to be ſolved remained as follows:

ought not to admit of partakers in their ſecret.

To invent a lock which cannot be opened but by the proprietor, of which the conſtruction ſhall be ſimple and eaſy, defended againſt all internal derangements, and, laſtly, capable of being readily opened by the proprietor.

Enunciation of the problem of a lock.

By reflecting on the conditions which a perfect lock ought to include, I was naturally led ſome years ago to this reſult; namely, That the ſecret of the lock ought to be variable at pleaſure*, or at leaſt that the ſecrets ſhould be ſo multiplied, that it ſhould be morally impoſſible for the workman who conſtructed the lock, or any other perſon, to conjecture which among them all the proprietor might have choſen at the time. I ſoon afterwards contrived ſeveral kinds of locks founded on this general principle.

Condition that the ſecret ſhould be variable at pleaſure.

By continuing to attend to this object, I ſoon found that others before me had arrived at the ſame reſult, and had publiſhed locks formed on the theory of combinations.

Others as well as the author had purſued this object.

As ſimplicity is one of the moſt eſſential conditions of this purſuit, and there are thouſands of ways of applying the principles of combinations to the conſtruction of locks, I thought it would not be without utility to explain the different reſults I have obtained. For it will not be till after having compoſed the moſt happy combinations, that we can hope to arrive at that ſimplicity, without which a lock of this kind would be more curious than uſeful.

Simplicity an eſſential requiſite.

I ſhall firſt deſcribe the lock which appears to me to be the beſt among thoſe I have thought of. I ſhall afterwards compare it with that invented in 1777, by Cit. Reguier, and de-

The author's lock to be compared with that of Reguier.

* This principle, which is appplied in the well-known lock of Braviali, was explained to me in converſation, much at large, by him, in 1797.

ſcribed

ſcribed as the moſt perfect, in the art of lock-making *(Serrurie)* in the Encyclopédie Méthodique. I ſhall ſhew that the lock of Reguier, as well as many others, contains an imperfection, or rather a radical defect, which eſcaped the inventor, and renders its other properties of little value *. It will be ſeen how I have avoided this defect in mine. And, laſtly, I ſhall conciſely deſcribe ſome other ſyſtems of locks which ſeem to offer particular advantages, but which, nevertheleſs, do not poſſeſs the ſame degree of perfection as that to which I have given the preference.

Deſcription of the new Lock.

Particular deſcription of the new lock:

The front of this lock conſiſts of a plate 230 millimetres † in length, 67 in width, and 4 or 5 thick. Along the middle line are diſpoſed five nuts or ſcrew heads, of 18 in diameter. In the centre of theſe nuts is a ſquare ſtem of 5 in the ſide, which ends in a ſcrew.

This ſquare ſtem carries immediately on the other ſide of the plate, a round plate of 38 in diameter and 3 thick, having a ſquare hole in the center, and having 24 teeth in its circumference, with ſpars between them equal to the teeth.

Upon this round plate is placed a cylindrical cup or ferril, having its bottom in contact with that wheel, and connected with it by the ſtem, which paſſes through a round hole in its center, and binds it down by the ſcrew nut. This ferril, of which the thickneſs is 2 and the height about 29, has a prominent ſtud at its flat face, which entering between any two teeth of the wheel, ſerves to vary at pleaſure the relative poſitions of the wheel and ferril at the time of ſcrewing them together. Laſtly, The ferril has 24 teeth cut in its edge, having a ſpace of about 4 between each. Theſe teeth are about 3 in depth, excepting one of the intervals which is cut to the depth of 6. We ſhall hereafter explain the uſe of this depth.

* Cit. Reguier has, nevertheleſs, the honour of being the firſt who nearly reſolved this problem.

† To avoid fractions, I have not reduced theſe meaſures. As the metre is 39.371 inches (Philoſ. Journ. II. 250), it will be ſufficiently near if the reader multiplies the numbers in the text by 4, and then cuts off two figures: Thus $230 \times 4 = 9.20$ inches, or $9\frac{1}{4}$ inches.—N.

The

The face of the lock is fitted up, as we have obferved, with five fcrew heads. The middle ftem, inftead of being provided with a wheel and ferril like the others, has a wheel of 45 in diameter, 4 in thicknefs, and has 24 teeth of the depth of 3.

Conftruction of a fecret lock acceffible only to the proprietor.

Fig. 1. Pl. II. reprefents the face of the lock. A B is 230, A A 67, and the five fcrew heads, R, are fpaced at equal diftances.

Fig. 2 reprefents the infide of the lock without its bolt. The plan of the four ferrils with their fcrews, together with the middle toothed wheel and the heads of the nuts, are vifible, but the four flat wheels are concealed beneath the ferrils.

The ferrils are inclofed in a rectangular box, 2 in thicknefs, and about 28 in height at the longer fides. The fhorter are 34 in height, and have an excavation or notch in them to the depth of 8. The fides of this box are at the diftance of 6 from the ferrils endwife, but only 2 as to the breadth. The diftance between the middle wheel and the neareft ferrils is alfo 2, but that between the contiguous ferrils is 5.

In the four inner angles of the box are fixed four pillars, P P P P, in thicknefs 6, in width 13, and about 35 above the plate. Thefe pillars have at their extremities a rectangular groove for the bolt to flide in, and they are fixed by fcrews to the long faces of the box, and by rivets to the end plates. The fquare frame of the box is faftened to the face by two cocks Z Z, which receive fcrews.

The bolt is conftructed as follows: A thin bar of iron is taken, of which the width C C, Fig. 3, is 67, and the length C D, which is variable towards D, is 310 from F to C. It is fitted with two edge bars C F, C F, of which the width is 5, and another C C, only 4. At the end which is to form the part of the bolt to fhoot out, a plate F F D D is fixed, of the fame breadth as C C, but in length arbitrary. The depth of the three edge bars is $6\frac{1}{2}$, and cannot be feen in the figure. That of the end plate depends on the ftrength intended to be given.

The two lateral edge bars C F have near their extremities a projection $x\,x'$, $x''\,x'''$, which exceed the edge bar by 3, and do not come down to the face of the bolt by an interval of 3. In thefe four fpaces the extremities of the pillar flide, and they themfelves receive the projections $x\,x'$, $x''\,x'''$ in their grooves.

Construction of a secret lock accessible only to the proprietor.

The diſtances of all theſe points from the extremity C C of the bolt are, namely, little $c\ x$ 20, $c\ x'$ 60, $c\ x''$ 226, $c\ x'''$ 274, C F 310.

In order to complete the bolt, it only remains to fix its rack and its four pins correſponding with the four ferrils.

The rack is a rectangle, of which the length is M N, Fig. 5, equal to 67, the breadth or depth M M′ 33, and the thickneſs, which could not be ſhewn in this figure, is 4, and may be ſeen in Fig. 3. This parallelopepidon is fixed perpendicularly upon the bolt at M′ N′, Fig. 3, by two projecting rivets K K, Fig. 5.

The rack is notched ſo as to form in its length M′ N′, 13 teeth of 5 in depth; ſo that it reſembles a comb with very ſhort teeth. The ſpaces are wider than the teeth, and they are rounded on the ſide which locks into the wheel.

The ſingle tooth or pin which is intended to correſpond to each of the ferrils, is formed of four parts, ſtand up at right angles, as is ſeen in Fig. 4. O H is the pin properly ſo called, rivetted perpendicularly in the bolt by the ſmall ſquare extremity H. I I are two ſmall wedges which precede the tooth. O H is 11, and I I 16.

The diſtance of the tooth or ſtud O from the line C C, Fig. 3, is 39; that of O′ is 118; that of O″ is 169; that of O‴ 248.

The little wedge which precedes the tooth, is 6 in length and 3 in height. We ſhall hereafter ſee the uſe of this.

From the poſition of theſe four ſtuds it follows, that when they are lodged preciſely in the thickneſs of the ferrils, the two extreme ferrils muſt have their long notch or aperture directly towards the extremities of the lock, and thoſe of the intermediate ferrils will, on the contrary, have their apertures both turned towards the middle wheel. We ſhall ſee alſo that it follows, that when the lock is open, the firſt pin O is out of the lock in the groove cut in the door; the tooth O′ is within the ſecond ferrol; the tooth O″ in the ſpace above the wheel; and the tooth O‴ within the fourth ferril. And the contrary is the caſe when the door is ſhut.

In order that the bolt may ſtop at the ſame point when the door is open, there is a ſmall protuberance X or X‴. The teeth O, O′, O″, O‴, are then ſufficiently diſtinct from the ferrils to allow of their being moved or ſet.

And

Construction of a secret lock accessible only to the proprietor.

And so likewise in order to stop the bolt always at the same point when shut, there is fixed at Q, near the short edge bar, a screw, the head of which stands higher than the face of the bolt, and stops it by taking against the box. When the bolt is required to be taken out, this screw is drawn back a little, and it passes clear of the edge bar. The same effect might be obtained by putting an interior stem to the handle, which shall stop the bolt when the door is locked, which takes place when the points X and X″ coincide with the faces of the pillars towards the knob Q. This knob is also of use to open or bolt the door within. The position of the knob Q is such, that the circumference of its screw stem is a tangent to the line X X.

The key remains to be described: This is a circular plate or disc, Fig. 6, of 50 in diameter and 3 in thickness. Its circumference is divided into 24 parts, carrying circularly the 24 letters of the alphabet, leaving out K. It has a handle in the form of a semiellipsis, to turn it with the finger and thumb, and this handle carries on the other side of the plate two small steel points T T, of about 3 in length, which enter into the holes of the screw heads. Lastly, This plate has an opening in its center about 20 in diameter, to allow the points to be visible when applied to the screw heads.

There are also marked on the face of the lock five small stars or lozenges, to serve as station-points for the key; and also five small holes near one of the two holes of the screw heads. These small holes serve as an indication to apply the key always in the same manner, by putting the same point in the same hole.

The position of the four stars corresponding with the ferrils, must be determined with great care and precision. For this purpose, the ferrils must be placed so that their openings shall be in a right line, and the holes of the screw heads in a vertical line. The bolt is then to be introduced by engaging it in the grooves of the pillars, so that the four teeth or pins may be lodged in the thickness of the ferrils. The turn-screw is then to be inserted successively in the holes of each screw-head; observing that the letter A, having its mark distinguished by a diamond, shall be turned towards the same side as the small indicative point placed on the screw-head, and then a point must be made on the face of the lock to correspond with the letter A of the key. At this point the star is to be made, which will be

Construction of a secret lock accessible only to the proprietor.

be very nearly in a vertical line: but it may easily be imagined that it will not be exactly so, unless the studs of the bolt should exactly fill the spaces, so as to leave no shake. The fifth star corresponding to the center wheel, is merely ornamental.

In this disposition of the lock we may affirm, that it is adjusted to the word A A A A.

In order to change the secret, and adjust the lock to the word *long*, for example, the first screw-head must be turned round to the left, until the letter L of the key coincides with the star. In the same manner the letter O is made to coincide with the star of the second screw-head; the letter N with that of the third; and that of the letter G with the fourth. The flat wheels remaining in that position, the ferrils are then to be disposed in a right line, and the secret will consist of the word *long*. The bolt is then to be put in its place, and the handle turned to advance its stop.

When the lock is to be shut, the screw-heads must be disposed so as to form the word *long*; and in this position the bolt may be moved backward and forward by turning the middle wheel. If one or more of the screw-heads be deranged, the bolt is locked fast. But in order to open it, the same disposition must be made, and the bolt may again be moved.

We may observe that, when the screw-heads are deranged, the bolt nevertheless is capable of moving through the space of 13, arising from the size of the ferrils; which affords an advantage for using it as a simple bolt, to be put backwards and forwards within the chamber.

(To be concluded.)

SCIENTIFIC NEWS, &c.

Cheap Method of producing Light.

MR. EZEKIEL WALKER informs me that he has discovered a cheap method of producing light, which seems to possess advantages much superior to the common modes of illumination. This light generates no smoke, and requires not the aid of snuffers.

On

On the Reflection of obscure Heat.

C. Pictet's experiment related in his essay on fire, to prove the reflexibility of obscure heat is well known. It consists in placing opposite to each other two concave metallic mirrors. In the focus of one is placed a hot but not luminous cannon-ball, in the focus of the other a very sensible air thermometer, and the latter is soon seen to rise rapidly. Pictet's experiments of reflected heat with mirrors.

The same philosopher has since published some other experiments on the same subject.

Having employed a lighted candle instead of the cannon-ball, he placed between the two foci a plate of very thin clear transparent glass, and which intercepted the light very little: the thermometer indicating the transmission of heat, stopped that instant. Glass intercepted the heat but not the light.

The two mirrors were placed at the distance of 25 metres (yards) one from the other, in order to determine whether the time of the propagation of the radiant heat from one focus to the other could be appreciated. A heated but not luminous ball was suspended at one of the foci, before which a screen was placed. At the instant that this obstacle was removed, the fluid of the thermometer, which was before perfectly at rest, began to move, and no sensible interval could possibly be perceived between the suppression and the effects of the transmitted heat. Distance of mirrors 25 metres.

C. Pictet relates this experiment in the Bibliothèque Britannique, in support of the opinion he had advanced in his essay on fire, that light and heat are not the same. This opinion has since been renewed by M. Herschell.—*Bulletin des Sciences, No.* 62. The heat was instantly transmitted.

On certain Facts commonly urged against the Doctrine of two Electric Fluids. By Cit. Tremery, *Engineer of Mines. (Bull. des Sciences, No.* 63.*)*

Among the facts which have been offered in support of Franklin's hypothesis of a single electric fluid, the most remarkable is the following, (commonly called *Lullin's* card, from the inventor of the experiment). The experiment of passing a shock through Lullin's card.

Having placed between two metallic conductors a card, which touches each of them, by its opposite faces, in two different

different points, a ſtrong electric charge is paſſed through the apparatus. At the inſtant of the operation, a luminous flaſh paſſes from the poſitive conductor, ſlides along the ſurface of the card, and perforates it oppoſite the negative conductor. This happens even when the card is perforated beforehand oppoſite the poſitive conductor.

penetrated oppoſite the neg. conductor.

From this fact it has been concluded, that in order to ſupport the theory of two fluids, we muſt ſuppoſe that one of them eſcapes from bodies and produces light, while the other remains inherent *. Cit. Tremery deſtroys this reaſoning by the following experiment:

This does not happen in rarefied air.

He places the card and the two conductors under the receiver of an air pump, and in proportion as the air is exhauſted, the place of fracture happens neareſt the poſitive conductor; and when the air is half exhauſted, the point is preciſely at the middle between the two conductors. At every diſcharge a luminous trace iſſues from each conductor along the ſurface of the card to the place of interſection.

Inferences.

From this experiment, Cit. Tremery concludes that the atmoſpheric air, in its ordinary ſtate, reſiſts the paſſage of the negative more than the poſitive fluid †. Theſe reſiſtances are diſtinguiſhed in different ratios, with regard to the two fluids, as the air becomes leſs denſe, and more rapidly with regard to the negative than the poſitive.

Hence Cit. Tremery deduces the general reſult, that the inſulating property of non-conductors cannot be the ſame for both electricities.

From this explanation, he thinks it eaſy to reconcile the theory of two fluids with the few facts oppoſed to it by his adverſaries.

* Or if one ſingle paſs from one conductor to the other, the attraction or power of the receiving conductor will be very oblique to the card at firſt, and moſt direct at laſt; whence the place of fracture is inferred to be at the receiving or minus extremity.

W. N.

† Yet a negative point diſcharges its electricity more eaſily than a poſitive. See Fig. 5, Pl. VIII.

W. N.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

DECEMBER, 1802.

ARTICLE I.

Description of an Improvement of Woulfe's Apparatus, by which the great Inconveniencies of that Method of connecting the Vessels are obviated. In a Letter from Mr. JOHN MURRAY.

Edinburgh, Sept. 18, 1802.

To Mr. NICHOLSON.

SIR,

I SEND you a drawing of an improvement in Woulfe's apparatus, which I believe I may recommend with confidence to chemists.

In the apparatus on the common construction it is extremely difficult, I might perhaps say impossible, to have a series of bottles connected by tubes which are ground air tight, and hence the necessity of employing luting to close the joinings, an operation extremely troublesome when done with accuracy. This difficulty of fitting the bent tubes to the bottles by grinding, arises from the construction of the apparatus itself; for if one leg of the tube be ground into the bottle from which it issues, it will be found scarcely possible to grind the other leg with accuracy into the other bottle, so as to fit it at the precise point

It is scarcely possible to connect vessels by air tight tubes without luting.

The cause why?

point at which the two bottles ſhall be on a level; and this difficulty is increaſed ſtill more when a ſeries of bottles are to be joined in this manner. The difficulty is indeed ſo great, that an apparatus of this kind perfectly air tight, though it would be of the greateſt advantage in many chemical operations, has perhaps ſcarcely ever been procured, and the attempt to have it made was relinquiſhed even by Lavoiſier.

Luting is moſt commonly uſed.

Various attempts have been made to obviate this inconvenience, but with ſo little ſucceſs, that the original apparatus in which the joinings are cloſed by a lute, is ſtill the one employed by chemiſts. Of the improvements that have been propoſed, I may remark that that by Dr. Hamilton, of which a deſcription is given in his tranſlation of Berthollet on Dyeing, and which does not ſeem to be ſufficiently known to chemiſts, is by far the moſt convenient. It may be ſimplified by having the bent tubes to iſſue from the receivers inſtead of being fitted to them by grinding, and for many purpoſes it anſwers exceedingly well. Its principal deficiency is, that no great degree of preſſure can be obtained in it, proportioned to the quantity of water put into the receivers.

Dr. Hamilton's excellent apparatus.

Cit. Girard's improvement very expenſive, and why:

Cit. Girard has lately propoſed another improvement, of which an account has been given in the 4th Vol. of your Journal (p. 41.) On ordering his apparatus at the glaſs manufactory, I found that it could not be made but at a very great expence. This was ſtated to be owing partly to the trouble which would attend the bending the long tube attached to the bottle, but ſtill more to the difficulty of giving this tube the ſame curvature, as the larger tube fixed in the bottle, into which the former is to be introduced. The execution of this by the mode Cit. Girard deſcribed, would not be ſo eaſy as he appears to have ſuppoſed it; and a number of bottles would probably have required to have been made, to obtain a ſeries of three or four adapted to each other.

This difficulty obviated by the author.

It ſometime afterwards occurred to me, that every difficulty might be obviated by a more ſimple method, which I have ſince found is extremely eaſy in the execution. The apparatus conſtructed on this method is repreſented in the annexed drawing. Plate XIII.

Deſcription. The contrivance of Girard applied perpendi-

A is an adopter ground into the bottle B, and connecting a retort with it. C is a ſtraight tube ground into an opening in the ſide of B, and into a ſimilar opening in the ſide of the bottle

bottle D. E is a bent tube paſſing from the neck of D, and fitted to it by grinding; and it is in the mode of connecting this with the ſucceeding bottle G, that the preſent improvement conſiſts. F is merely a common Woulfe's bottle with two necks. But in the firſt neck a tube G is ſoldered, when the bottle is made, which deſcends to within 1½ inch of the bottom, the height of the bottle being ſix inches. The long leg of the tube E deſcends into this tube, reaches beyond the extremity of it, and has at its termination a ſlight curve. It is obvious that when water is put into the bottle F, ſo that the extremity of G is immerſed in it, if any vapour or gas paſs from D, to F by E, it will be diſcharged from the extremity of the latter, and from the degree of bending will be thrown beyond the extremity of G, and as this is immerſed in the fluid, the gas cannot eſcape, but whatever part of it is not abſorbed, muſt be collected in the upper part of F. It paſſes in a ſimilar manner to H, and from H to I, being in each bottle tranſmitted through a portion of water. Any permanent gas, not abſorbed, paſſes through the bent tube fitted to I, and may be collected in a jar placed in a ſmall pneumatic trough.

cularly at the outer part of Woulfe's tubes.

The advantages of this apparatus are obvious. All the joinings are air tight without lute, and the tubes are at the ſame time ſo far free, that they are in no danger of being broken by any ſlight agitation or derangement. In making it, it is moſt convenient to have the tubes firſt ground in, and when perfectly dry to have them bent by the blow pipe.

Advantages of the apparatus. It is air tight and ſafe.

The firſt bottle in the apparatus being deſigned to receive any fluid condenſed in it during diſtillation, has no tube of ſafety, becauſe at the commencement at leaſt of the diſtillation, there would be no fluid in which its extremity would be immerſed. On this account it is requiſite that this firſt bottle ſhould be connected with the ſecond by a ſtraight tube, as repreſented in the drawing, and not by a bent tube as in the other bottle, elſe upon any condenſation in the retort or firſt bottle, the liquor would ruſh backward from the ſecond. In this ſecond bottle a tube of ſafety is fitted by grinding, which is neceſſary to guard againſt condenſation. In the other bottle this is not neceſſary, as the open tubes are ſufficient to admit air.

Obſervations.

The only inconvenience in this apparatus is, that the fluid in the bottles is liable to be forced up in the open tubes, in

This apparatus and Girard's are liable to have the fluid preſſed

upwards between the tubes. the tube G for example in the bottle F, by the preſſure of the gas accumulated in it from the reſiſtance of the column of water in the other bottles; and this even proceeds ſo far that part of the liquor is forced out entirely. This can in ſome meaſure be obviated by diminiſhing the quantity of fluid in the laſt bottles, or by introducing into them little more than is ſufficient to immerſe the extremities of the tubes. In this way however the preſſure obtained, by which the abſorption of a gas is promoted, is very inconſiderable. The ſame inconvenience is preſent in the apparatus of Cit. Girard, and can be obviated only in the ſame mode. In the preſent apparatus, another improvement may be introduced, by which it is more effectually removed, that of having the ſtraight tubes which are ſoldered into the bottle of a greater length. This is repreſented in the drawing at K L, the open tube A riſing five or ſix inches above the neck of the bottle. It is rather more difficult to ſolder in the tube in this mode, but the advantage from the increaſed height is ſo great, that the apparatus ſhould perhaps always be conſtructed in this manner.

Remedy, by lengthening the external tube.

How to convert a common Woulfe's apparatus into the preſent.

Inſtead of fixing the ſtraight tubes into the bottles when they are made, they might afterwards be fitted in by grinding, and then a common Woulfe's apparatus could be eaſily converted into this improved one. The other mode however is preferable, as we are more certain of having the apparatus air tight, and it is ſo eaſily done that it adds little to the expence.

I am, with reſpect,

your's, &c.

J. MURRAY.

II. *Obſervations*

II.

Obſervations and Experiments upon Oxigenized and Hyperoxigenized Muriatic Acid; and upon ſome Combinations of the Muriatic Acid in its three States. By RICHARD CHENEVIX, *Eſq. F. R. S. M. R. I. A. From the Philoſophical Tranſactions for* 1802.

(Concluded from Page 185.*)*

Third Species. Hyperoxigenized Muriate of Barites.

THE earthy baſes ſeem to follow, in the order of affinities for this acid, at a great diſtance from the alkalis. They are all ſuperſeded by the two juſt mentioned; and it is much more difficult to accompliſh their union with the acid, than is the caſe with potaſh or ſoda. The moſt advantageous method is, to pour warm water upon a large quantity of this earth, procured by Mr. Vauquelin's method; and to cauſe a current of oxigenized muriatic acid to paſs through the liquor, kept warm; ſo that the barites already diſſolved being ſaturated, a freſh portion of it may be taken up by the water, and preſented in a ſtate of great diviſion to the acid. This ſalt is ſoluble in about four parts of cold, and leſs of warm water. It cryſtallizes like the muriate of this earth; and reſembles it ſo much in ſolubility, that I could not ſeparate them effectually by cryſtallization repeated ſeveral times. At firſt, indeed, I deſpaired of ever obtaining any of the earthy hyperoxigenized muriates in a ſtate ſufficiently pure for analyſis. If we conſider them as a genus diſtinct from the alkaline hyperoxigenized muriates, a leading character may be, their great reſemblance to their reſpective ſpecies of earthy muriates. I thought, however, that I might, if not by direct, at leaſt by double affinity, decompoſe the one without the other; and phoſphate of ſilver occurred to me as the moſt likely agent. If phoſphate of ſilver be boiled with muriate of lime, of barites, &c. a double decompoſition enſues; and muriate of ſilver, together with phoſphate of the earth, both inſoluble, are precipitated. To increaſe the action, the phoſphate of ſilver may be diſſolved in a weak acid, ſuch as the acetous; and, though the earthy phoſphate be at firſt retained in ſolution, it will be ſeparated

Hyperox. mur. of barites.

Soluble; cryſtallizable.

Purification of earthy hyp. ox. muriates; (not by diff. of ſolubility.)

by phoſphate of ſilver, which by double affinity decompoſes the muriates, and leaves the hyperox. muriates in ſolution alone.

The silver must be free from copper.

separated by expelling the acid. The only condition absolutely necessary is, that the silver employed be free from copper. For, in preparing phosphate of silver by phosphate of soda, and by nitrate of silver thus impure, copper would be thrown down by the phosphoric acid; and the phosphate of copper would be afterwards decomposed by muriate of lime. Muriate of copper would therefore remain with the earthy hyperoxigenized muriates; or, what is still worse, a part of the muriatic acid being easily expelled from oxide of copper, the hyperoxigenized muriatic acid would be driven off from its basis, by the more powerful agency of the former. This salt has all the properties enumerated as belonging to the genus of hyperoxigenized muriates; and, with heat, the acid is expelled by all acids above the benzoic. I had hoped that, without distillation, I could procure the acid from the salt by means of sulphuric acid, which would have left an insoluble salt with barites; but hyperoxigenized muriatic acid is so easily decomposed by light, that I have not yet obtained it, to my satisfaction, disengaged and pure. A fact well worthy of attention is that the stronger acids disengage this acid with a flash of light, more frequently from the earthy than from the alkaline hyperoxigenized muriates; a phenomenon which, I suppose, depends upon the relative proportionate affinities, and consequently the greater rapidity of the disengagement. But, where all is hypothesis, it is useless to draw any inference from a single fact.

Properties of the earthy hyp. ox. mur.

emit light more frequently than those with alk. basis.

Component parts, hyp. ox. mur. barites.

The proportions of this salt are,

Hyperoxigenized muriatic acid	47
Barites	42,2
Water	10,8
	100,0

Hyp. ox. mur. strontia.

Fourth Species. Hyperoxigenized Muriate of Strontia.

The foregoing observations apply to the formation of this salt, to the mode of obtaining it pure by phosphate of silver, to its conduct with the acids, to the rank of its acid in the order of affinities, and to its other properties. It is deliquescent; and more soluble in alcohol than muriate of strontia. It melts in the mouth immediately, and produces cold. Its crystals assume the shape of needles.

Deliquescent, and more soluble in alcohol than the muriate.

It

It is composed of, — Comp. parts hyp. ox. mur. of strontia.

Hyperoxigenized muriatic acid	46
Strontia	26
Water	28
	100.

Fifth Species. Hyperoxigenized Muriate of Lime. — Hyp. ox. mur. lime very deliquescent, and soluble in alcohol.

This salt is obtained pure, in the same manner as the other earthy salts. It is extremely deliquescent; liquifies at a low heat. by means of its water of crystallization; and is very soluble in alcohol. It produces much cold, and a sharp bitter taste in the mouth.

It is composed of, — Comp. parts hyp. ox. mur. of strontia.

Hyperoxigenized muriatic acid	55,2
Lime	28,3
Water	16,5
	100,0.

Sixth Species. Hyperoxigenized Muriate of Ammonia. — Hyp. ox. mur. ammonia.

From the property which oxigenized muriatic acid possesses of decomposing ammonia, this combination may be thought paradoxical. For, how can an acid much more active than oxigenized muriatic acid exist with ammonia, which is destroyed by the latter? But this argument may be opposed by the sum of affinities that act in either case. If the affinity of composition of oxigenized muriatic acid and of ammonia, together with the affinity of oxigenized muriatic acid for ammonia, to form oxigenized muriate of ammonia, be not more powerful than the affinity of oxigen for hidrogen, of azote for caloric, and of muriatic acid for ammonia, the divellent affinities will prevail; and this is what actually happens. But, although oxigen may be held with less force of attraction in oxigenized than in hyperoxigenized muriatic acid, yet the affinity of the latter acid for ammonia may increase in a much greater ratio, and favour the quiescent affinities. If carbonate of ammonia be poured into any earthy salt of this genus, a double decomposition takes place; and hyperoxigenized muriate of ammonia is formed. This salt is very soluble in water, and in alcohol. It is decomposed at a very low temperature, and

The ox. mur. acid decomposes ammonia by union of the hidrogen and oxigen; because this last attraction, &c. exceeds thosewhich might form ox. mur. of ammonia. But the hyp. ox. mur. acid does not; because the attraction of compos. to form hyp. ox. mur. of ammon. is strongest. This salt formed by double affinity: *e. g.* carbonate of amm. + hyp. ox. mur. of earth afford carb. of earth and hyp. ox. mur. of am.

It is very soluble in water and alcohol, and decomposed at low temperature.

and gives out a quantity of gas, together with a smell of hyperoxigenized muriatic acid. Such a smell is doubtless owing to the great quantity of oxigen contained in the acid; it being more than is necessary to combine with the quantity of hidrogen contained in the alkali, and therefore some of the acid is disengaged, without decomposition. All the attempts I have made to ascertain the proportions of its principles, have been fruitless. The formation and existence of this salt, as I before said, are very strong proofs of what I have advanced respecting the state in which hyperoxigenized muriates at first exist; and very fully prove the different degree of affinity exercised by each acid toward the basis.

Component parts unknown.

Seventh Species. Hyperoxigenized Muriate of Magnesia.

Hyp. ox. mur. of magnesia.

Its chemical and physical properties are nearly the same with those of the 5th species, only that, in addition to the other bases, lime and ammonia cause a precipitate in this salt. Its proportions are,

Component parts.

Hyperoxigenized muriatic acid - -	60
Magnesia - - - - - - -	25,7
Water - - - - - - - -	14,3
	100,0

Eighth Species. Hyperoxigenized Muriate of Alumina.

Hyp. ox. mur. of alumina.

I put some alumina, precipitated from muriate of alumina, and well washed, but still moist, into a Woulfe's apparatus, disposed as for the other earths, and sent a current of oxigenized muriatic acid gas through the liquor. The alumina shortly disappeared; and, upon pouring sulphuric acid into the liquor, a strong smell of hyperoxigenized muriatic acid was perceivable. When I attempted to obtain the salt pure, by phosphate of silver, in the usual way, I found nothing in solution but hyperoxigenised muriate of silver *; and all the hyperoxigenized muriate of alumina had been decomposed. This salt, however, appears to be very deliquescent, and is

It is decomposed by phosphate of silver, is deliquescent, and soluble in alcohol.

* This salt shall be particularly mentioned and described in another part of this Paper. For the present, it is sufficient to say, that it is very soluble in water; and, in that property, as in many others, is totally different from muriate of silver.

soluble

ſoluble in alcohol; but I could not aſcertain the proportion of its principles, becauſe I did not obtain it ſufficiently free from the ſimple muriate.

Ninth Species. Hyperoxigenized Muriate of Silica.

Hyp. ox. mur. ſilica does not exiſt.

I am inclined to think this ſalt does not really exiſt A current of oxigenized muriatic acid, ſent through ſome ſilica which had been precipitated from an acid by ammonia, and collected moiſt from the filter, did not ſeem to diſſolve any portion of it. In all barites and ſtrontia, prepared according to Mr. Vauquelin's method, a portion of ſilica from the crucibles is attacked, and taken up, by whatever acid thoſe earths may afterwards be diſſolved in: and, in all potaſh of commerce, there is ſome ſilica; but I have never perceived that any portion of this earth had been diſſolved by this acid.

General obſerv. on the earthy hyp. ox. mur.

The very ſmall portion of earth which, in attempts to form the different ſpecies of this genus of ſalts, is taken up by acids, and the ſtill ſmaller portion of the ſalt ſo formed, which is really in the ſtate of hyperoxigenized muriate, render the operation ſo tedious, that I have confined myſelf to form what was neceſſary to determine their analyſis, in ſuch a manner as I believe to be nearly accurate. It cannot, therefore, be expected that I make myſelf reſponſible, without a right of appeal to further experiments, for the accuracy with which the cryſtalline forms, and other phyſical properties, may have been ſtated. It is impoſſible to obtain ſatisfactory cryſtals from a very ſmall portion of ſalt; and I have attached myſelf more particularly to chemical than to phyſical characters, as being a much more important and certain mode of determination. For the ſame reaſon, I have not examined the combination of the new and rarer earths with this acid. But I do not doubt, that whatever chemiſt undertakes a further inveſtigation of theſe extraordinary bodies, will be amply repaid for his labour.

All muriates loſe ſome acid by ignition.

I have mentioned, in a former part of this Paper, that all muriates loſt a portion of their acid at a red heat. I expoſed one hundred parts of muriate of potaſh, in a crucible, to a red heat, for ſome minutes, and found that they loſt five. I diſſolved them in water, and they manifeſted alkaline properties. Treated by nitrate of ſilver, they gave a precipitate, which ſhewed one per cent. leſs of muriatic acid, than 100 parts

parts of the ſame ſalt that had not been expoſed to fire. A violent heat may be neceſſary to expel the laſt portion of water of cryſtallization from certain ſalts, as we know particularly is the caſe with ſulphate of lime. But, if any of the acid can be expelled at the ſame temperature, there is no longer any certainty. The quantity of water, as ſtated by different chemiſts, varies much; and, from ſome experiments I have made, I do not believe it to have been accurately determined. The method I uſed to aſcertain this, was, as follows: I expoſed a given quantity of the ſalt to a violent heat, and noted its loſs of weight. I then precipitated, by nitrate of ſilver; and thus knew, how much the quantity of muriatic acid which this ſalt contained, was leſs than that in a like portion which had not been expoſed to heat. I ſubtracted the difference in this quantity, from the total loſs of weight in the ſalt expoſed to heat; and the remainder I conſidered as water. It was upon reſults obtained in this manner, that I founded many of the proportions I have given in this Paper.

Whence the water of cryſtallization is inaccurately determined. If the acid of the ſalt be determined by precip. with ſilver before and after ignition, it may be known how much of the loſs by the latter proceſs is acid, and how much water.

It is ſtated in the tables of Bergman, corrected by Dr. Pearſon, that lime and ſtrontia prefer acetous to arſenic acid. But arſenic acid can expel hyperoxigenized muriatic acid from its baſis, although the acetous cannot act in the ſame manner; therefore, this order of affinities is erroneous. It was not till lately, that we had potaſh and ſoda ſo pure as to be relied upon in delicate experiments; and it is not ſurpriſing that we find miſtakes with regard to their taking the acid from barites, ſtrontia, and lime. But real potaſh and ſoda both precipitate even barites from hyperoxigenized muriatic acid. If ever it becomes eaſy to obtain hyperoxigenized muriate of barites, we may prepare that earth from it in the humid way, and more near to purity, than in the method propoſed by Vauquelin.

Obſervations on elective attractions.

METALLIC COMBINATIONS OF MURIATIC ACID, IN ITS DIFFERENT STATES.

Metallic muriatic ſalts.

The action of hyperoxigenized muriatic acid upon metals, is, as may well be expected, rapid, and without diſengagement of gas. It appears to diſſolve every metal, not excepting gold and platina. If the metal be preſented to the acid at the moment when it is diſengaged from the ſalt, inflammation enſues; and the phenomena of light and heat vary according

Hyp. ox. mur. acid diſſolves all *metals:* inflames them and produces muriates.

cording to the metal; but the salts thus produced are merely muriates. In order to form real hyperoxigenized muriates, it is necessary to take the metal in its fullest state of oxidizement, and combine it with the acid, either by double decomposition, or by passing a current of oxigenized muriatic acid gas through the oxide suspended in water. The acid is thus separated into muriatic and hyperoxigenixed muriatic acid; and, in these states, combines with the metallic oxide. The metallic hyperoxigenized muriates are different, in every respect, from the metallic muriates. Red oxide of iron is dissolved with difficulty. Oxide of copper more easily. Red oxide of lead exhibits the same appearances, during its combination with this acid, as with nitric acid. When nitric acid is poured, even in excess, upon red oxide of lead, only a part of the oxide is dissolved, unless heat be applied; and what remains becomes a blackish brown powder. But, if metallic lead be added, in a just proportion, all the red oxide disappears, and none of the brown powder is formed; neither is there any disengagement of nitrous gas, when the metallic lead is dissolvrd. The precipitates caused in either case, by pouring an alkali into the nitric solution, are yellow. Hence it appears, that red oxide of lead contains too much oxigen to be dissolved by nitric acid. One part of the oxide takes up the excess of oxigen, and becomes brown; while the portion which loses oxigen, becomes yellow, and is soluble in nitric acid. The presence of metallic lead promotes the total solution of the red oxide, by taking up the superabundant oxigen. I found that a current of oxigenized muriatic acid gas, like the nitric acid, dissolved a part of the red oxide, and caused the brown powder to be formed, upon which it could not act. Hyperoxigenized muriate of lead is much more soluble than muriate of lead; and the acid is very slightly attracted by the basis.

It must combine with the maximum oxides, to produce h. ox. muriates.

These greatly differ from muriates.

Habitudes of metallic oxides. Iron. Copper. Lead.

Red oxide of lead is too oxigenated to be wholly dissolved in nitric acid. Part becomes more oxigenated and the other part dissolves.

Ox. mur. acid acts similarly.

Hyp. ox. mur. of lead: soluble.

But, of all the metallic salts formed by the combination of the muriatic acid, in any of its different states, none so much deserve attention as those which have for their bases, the oxides of mercury. The nature of the salts which result from the combination of common muriatic acid with the different oxides of this metal, has been stated in the most contradictory manner, by different chemists. But, as the knowledge of hyperoxigenized muriatic acid has thrown some light upon the true

Mercurial muriates.

Calomel and corrof. fublimate.

true ftate of calomel and corrofive fublimate *, I muft beg leave to dwell at fome length upon this important part of my fubject.

It would be ufelefs to repeat the opinions of the old authors, who have treated of corrofive fublimate and of calomel. They are to be found in the works of thofe refpective chemifts, and I muft refer to them for particulars.

Opinions refpecting corrofive fublimate.

In the Memoirs of the Academy of Sciences of Paris, for 1780, we find a Paper of Mr. Berthollet, upon the caufticity of metallic falts; in which he appears to think, that the acid in corrofive fublimate is in the ftate of what was then called dephlogifticated marine acid. In 1785, when he had examined the oxigenized muriatic acid with more care, he renounced his former opinion; and gave the reafons why he no longer adhered to it. Some late experiments of Mr. Prouft fhew, that this chemift thinks as Mr. Berthollet now does. And thefe may be ranked among the firft of modern authorities.

Fourcroy confiders it as hyperox. muriate of mercury;

Notwithftanding thofe opinions, Mr. Fourcroy, in his *Syfteme des Connoiffances chimiques*, ftill confiders corrofive fublimate as a hyperoxigenized muriate of mercury; and defigns it throughout by that name †. This chemift, one of the founders of the methodical nomenclature, is too well acquainted with its principles, to apply the term hyperoxigenized muriate to any thing but a combination of hyperoxigenized muriatic acid. It is evident, therefore, that he confiders the portion of oxigen, which, in equal quantities of corrofive fublimate and calomel, is greater in the former, to be combined with the acid, and not

* I regret very much, that I am under the neceffity of ufing thefe unmeaning terms. But the French nomenclature has made no diftinction between falts formed by metallic oxides in different ftates of oxidizement, except by the colour, which is an extremely defective and unmeaning method. At all events, this metal is fo uncomplaifant as to retain the white colour, in its different oxides combined with muriatic acid. I prefer, however, ufing the old name, to propofing any provifional fubftitute that might be found defective. This will be farther explained in *Remarks upon Chemical Nomenclature.*

† I have faid before, that this acid was talked of by many chemifts, as if the exiftence of it had really been proved.

with

with the oxide of mercury. As ſoon as I have ſtated ſome experiments that prove Mr. Fourcroy's opinion to be erroneous, and endeavoured to eſtabliſh the analyſis of corroſive ſublimate and of calomel, I ſhall take notice of a ſalt hitherto unknown, which really is hyperoxigenized muriate of mercury.

but erroneouſly. For,

I took a portion of corroſive ſublimate, and precipitated by potaſh. The liquor was filtered; and, upon being tried, nothing but muriate of potaſh was found. No re-agent could diſcover the ſmalleſt trace of hyperoxigenized muriatic acid.

Corroſ. ſub: precipitated by potaſh gave muriate of potaſh and no hyp. ox. m. acid.

Sulphuric, nitric, phoſphoric, and many other acids, poured upon corroſive ſublimate, did not diſengage either muriatic, or hyperoxigenized muriatic acid. Nitrate of ſilver, poured into a ſolution of corroſive ſublimate, gave an abundant white precipitate.

The acids diſengaged nothing from it.

From theſe experiments it is evident, that muriatic acid, not hyperoxigenized muriatic acid, is combined with the oxide of mercury in corroſive ſublimate.

Conſeq. muriatic and not h. ox. m. acid exiſts in cor. ſub.

To determine the proportions of this ſalt, I took one hundred parts, and precipitated by nitrate of ſilver. I then took another hundred, and precipitated by potaſh. The reſult of theſe two experiments was ſuch as to eſtabliſh the proportions of corroſive ſublimate as follows:

Component parts by precipitation with potaſh and with nitrate of ſilver.

Oxide of mercury - - - -	82
Muriatic acid - - - - -	18
	100.

But, the acid of this ſalt not being charged with a ſuperabundance of oxigen, we muſt look for the exceſs in the metallic oxide. I took 100 grains of mercury, and diſſolved them in nitric acid; then poured in muriatic acid; and, at a very gentle heat, evaporated to dryneſs. I afterwards ſublimed, in a Florence flaſk, the ſalt that remained, and obtained 143,5 of corroſive ſublimate. But, 143,5 of corroſive ſublimate contain 26 of acid; which will leave 117,5 for the mercurial oxide; and, if 117,5 contain 100 of mercury, 100 of the oxide will contain 85. Therefore, the oxide of mercury in corroſive ſublimate, is oxidized at the rate of 15 per cent.

To meaſure the oxigen, 100 grs. of mercury were diſſolved in nitric acid; mur. acid was then poured in, and the ſalt (cor. ſub.) dried and ſublimed = 143.5; of which 18 per cent. was acid, and conſeq. 15 per cent. of the oxide was oxigen.

Component parts of calomel.

To determine the proportions in calomel, I diſſolved 100 grains of it in nitric acid. The phenomena of the ſolution have been ſo accurately deſcribed by Mr. Berthollet, that I ſhall not repeat them. I precipitated by nitrate of ſilver; and obtained a quantity of muriate of ſilver correſponding with 11,5 of muriatic acid. The oxide of mercury I obtained apart. Therefore, calomel is compoſed of,

Oxide of mercury - - -	88,5
Muriatic acid - - -	11,5
	100,0.

Calomel was converted into cor. ſubl. by boiling in nitro-mur. acid, evapor. and ſubl. It had gained weight, of which, from the former expts. the proportions of acid and of oxigen were known;

To aſcertain the ſtate of oxidizement of the oxide in calomel, I took 100 grains, and boiled them with nitro-muriatic acid; then evaporated very ſlowly, and ſublimed as above. The calomel was totally converted into corroſive ſublimate, and weighed 113. But 113 of corroſive ſublimate contain 20,3 of muriatic acid, of which, 11,5 were originally in the calomel. The total addition of weight was 13. But the quantity of acid in theſe 13, amounts to 20,3 — 11,5 = 8,8. Therefore, 13 — 8,8 = 4,2, remain for that part of the additional weight which is oxigen. On the other hand, 100 of calomel contain the the ſame quantity of mercury as 113 of corroſive ſublimate, = 79. Theſe 79, with 11,5 of acid, are equal to 90,5, and leave 9,5 for the quantity of oxigen contained in calomel. It would appear, from theſe experiments, that corroſive ſublimate contains 6,5 per cent. more acid, and but 2,8 per cent. more oxigen, than calomel. But this quantity of oxigen is combined with a much greater proportion of mercury; and forms an oxide of a very different degree of oxidizement. For, 88,5 : 9,5 : : 100 : 10,7. Therefore, we may eſtabliſh the following comparative table:—

whence the proportion of oxigen in the oxide is deduced, viz. 10.7 per cent.

CALOMEL.

Comparative ftatement.

CALOMEL.		CORROSIVE SUBLIMATE.	
The oxide of mercury in calomel is compofed of,		The oxide of mercury in corrofive fublimate is compofed of,	
Mercury - - -	89,3	Mercury - -	85
Oxigen - - -	10,7	Oxigen - - -	15
	100,0.		100.
And calomel is compofed of,		And corrofive fublimate is compofed of,	
Mercury 79 } oxide of mercury	88,5	Mercury 69,7 } oxide of mercury	82
Oxigen 9,5 }		Oxigen 12,3 }	
Muriatic acid -	11,5	Muriatic acid -	18
	100,0.		100.

Thefe proportions are different from thofe given by Lemery, Geoffroy, Bergman, &c. But, without calling in queftion the accuracy and fkill of thefe chemifts, it is fair to affert, that the pure materials ufed by modern chemifts, are more likely to lead to fure refults, than the impure re-agents of the ancients.

The moft oxig. oxides faturate the largeft quantity of acid.

In thefe falts we find another inftance, that, in proportion as metallic oxides contain a greater quantity of oxigen, they require a greater quantity of acid to enter into combination with them.

Obfervations to fhew that the foregoing proceffes were found the moft accurate and concife.

The method I have followed, to afcertain the proportions juft ftated, may appear, at firft view, not to be the fhorteft that I might have adopted. But I have tried others, and I have found none fo accurate. It is impoffible, fynthetically, to convert a given quantity of mercury into calomel, in fuch a manner as to be certain that none of it is in a different ftate from that required. And, if we would attack calomel analytically, the action of the alkalies, without which we cannot proceed, is fuch as to alter the nature of the oxides. I have alfo made many comparative experiments, by diffolving calomel in nitro-muriatic acid, (which converted it into corrofive fublimate,) and then precipitating by ammonia; but I have not found thefe trials fo fuccefsful as thofe I have defcribed. The nature of the precipitate from corrofive fublimate by ammonia, certainly differs, according to the excefs of acid that may be prefent; and mercury feems to have the power of exifting in many degrees of combination with oxigen. The only

precaution abſolutely neceſſary, in this mode of operating, is, that while the mercurial ſalt is in an open veſſel, it ſhould not be expoſed to a degree of heat capable of volatilizing any part of it.

London Pharmacopœia preſcribes too much mercury in the formula for cor. ſub. But this is prudent.

The quantity of mercury ordered in the London Pharmacopœia, to convert corroſive ſublimate into calomel, is 9 pounds of mercury for every 12 pounds of corroſive ſublimate. But, from the above experiments, it would appear, that a ſmaller quantity of mercury might ſtrictly anſwer. However, from the reſults of minute inveſtigation, we ſhould not conclude too haſtily upon preparations on the great ſcale; and, I rather think, that the exceſs of mercury ordered by the Pharmacopœia is a uſeful precaution.

In attempts to reduce the mercurial ſalts by other metals, iron failed, zinc threw down mercury, and copper precipitated calomel from cor. ſub.

In my experiments, I attempted to reduce, by means of copper, iron, or zinc, the mercury contained in the mercurial ſalts. Iron did not anſwer the purpoſe: zinc precipitated the mercury a little better; and copper produced a change which I did not expect. If a bit of copper be put into a ſolution of corroſive ſublimate, a white powder ſhortly falls to the bottom; and that powder is calomel. When waſhed, it does not contain an atom of copper, or of corroſive ſublimate.

Calomel is the ſame whether produced in the humid or dry way: and alſo cor. ſub.

Before I conclude theſe conſiderations, I muſt ſay, that whether calomel be prepared in the dry or in the humid way *, it

Obſervations on Scheele's humid proceſs for calomel. It is not pure, but contains a ſub-nitrate of mercury.

* By the humid way, I do not mean preciſely the method of Scheele. That chemiſt deſires us to boil the acid with the mercury, after they have ceaſed to act upon each other at a low temperature. By this method, the nitric acid takes up an exceſs of mercurial oxide; and the nitrate of mercury thus formed, precipitates by water. Therefore, when this nitrate of mercury is poured into the dilute ſolution of muriate of ſoda, according to the formula of Scheele, the action, on the part of the ſolution, is twofold.

1ſt. The water acts upon one part, and precipitates an oxide, or rather an inſoluble ſub-nitrate of mercury. And,

2dly. A double decompoſition takes place between the nitrate of mercury and the muriate of ſoda. It is with reaſon, that the medical world have ſuppoſed the calomel of Scheele to be different from that prepared in the humid way; for it is, in fact, calomel, plus an inſoluble ſub-nitrate of mercury. In the firſt part of Scheele's proceſs, there is diſengagement of nitrous gas, together with oxidizement and ſolution of ſome of the mercury. When he boils the acid upon the remaining mercury, there is no further diſengagement of gas;

it does not ſeem to differ chemically; nor does it contain any ſenſible portion of water of cryſtallization. The ſame may be ſaid of corroſive ſublimate.

It now remains to ſpeak of the real hyperoxigenized muriate of mercury. I paſſed a current of oxigenized muriatic acid gas through ſome water, in which there was red oxide of mercury *. After a ſhort time, the oxide became of a very dark brown colour; and a ſolution appeared to have taken place. The current was continued for ſome time; and, when I thought that a ſufficient quantity of the oxide had been diſſolved, I ſtopped the operation. The liquor was evaporated to dryneſs; and the ſalt was thus obtained. There evidently was in the maſs a great proportion of corroſive ſublimate, as might be expected, from what I had obſerved to take place in the formation of the other ſalts of this acid; but, by carefully ſeparating the laſt formed cryſtals, I could pick out ſome hyperoxigenized muriate of mercury. I then cryſtallized it over again; and, in this manner, I obtained it nearly pure. This ſalt is more ſoluble than corroſive ſublimate: about four parts of water retain it in ſolution. The ſhape of its cryſtals, I cannot well

Hyper-ox. mur. of mercury: obtained by paſſing ox. m. gas through water containing red oxide of mercury.

It is more ſoluble than cor. ſub. and is ſeparable by cryſtallization.

gas; yet more mercury is diſſolved. The nitrate of mercury, therefore, rather contains an oxide leſs oxidized after ebullition than before it. The true difference is in the ſub-nitrate of mercury, precipitated, as I before ſaid, by the water in which the muriate of ſoda was diſſolved. And the orange-coloured powder, which remains after an attempt to ſublime Scheele's calomel, is to be attributed to the ſame cauſe. To prepare calomel in the humid way, uniform as to itſelf, and in all reſpects ſimilar to that prepared in the dry way, it is neceſſary, either to uſe the nitric ſolution before it has boiled, or to pour ſome muriatic acid into the ſolution of muriate of ſoda, previouſly to mixing it with the boiled ſolution of nitrate of mercury. In the firſt caſe, no precaution is neceſſary; and, in the latter, the oxide of mercury, which the nitrate of mercury has, by boiling, taken up in exceſs, finds an acid which is ready to ſaturate it. All the mercurial oxide being thus converted into calomel, none of that ſub-nitrate of mercury can be preſent.

Preparation of calomel in the humid way.

The objections made by a medical gentleman againſt Scheele's calomel, when this paper was read before the Royal Society, led me to reconſider the ſubject, and to undertake the inveſtigation detailed in this note.

* I uſed either of the red oxides of mercury indiſcriminately.

determine. When ſulphuric, or even weaker acids, are poured upon it, it gives out the uſual ſmell of hyperoxigenized muriatic acid; and the liquor becomes of an orange colour. This is a ſufficient proof, that corroſive ſublimate is not a hyperoxigenized muriate of mercury.

Dark brown oxide of mercury not taken up, ſuppoſed to be different from the red oxide.

I have juſt mentioned that, in the formation of this ſalt, the oxide of mercury, which was not diſſolved by the acid, became of a very dark brown colour. I procured a portion of this oxide, which ſeemed different from the red oxide. It however retained the form, and the cryſtalline appearance, of the latter. It was ſoluble in nitric acid, without diſengagement of gas; and was precipitated from it, in a yellow oxide, by all the alkalis, except ammonia. It formed corroſive ſublimate with muriatic acid; and the precipitate by the alkalis, was the ſame as that from corroſive ſublimate, made with the red oxide. Yet I am inclined to think, that the dark brown oxide differs in ſome eſſential point from the red; but I have not yet made ſufficient experiments to prove this opinion. At all events, the preſent object being to examine the mercurial oxides only as combined with muriatic acid, it would be foreign to the purpoſe, to enter into too minute an inveſtigation of the other ſtates of the metal. This, and ſome other objects hinted at in this Paper, muſt be reſerved for future inquiry.

Hyper-ox. mur. of ſilver

In treating the earthy hyperoxigenized muriates with phoſphate of ſilver, as I mentioned before, I obſerved that the liquor ſometimes contained in ſolution oxide of ſilver; which, upon examination, I found to be combined with hyperoxigenized muriatic acid. As the ſalt which is thus formed is different, in every reſpect, from ſimple muriate of ſilver, it may be of ſome importance to conſider it with attention. In the firſt place, it will afford the moſt convincing proof of the difference between muriatic and hyperoxigenized muriatic acid; and, in the next place, it particularly deſerves to be remarked, for poſſeſſing, in the moſt eminent degree, one of the great characteriſtic features of the genus to which it belongs.

ſoluble in 2 parts warm water, and cryſtall. by cold:

Hyperoxigenized muriate of ſilver is ſoluble in about two parts of warm water; but, by cooling, it cryſtallizes in the ſhape of ſmall rhomboids, opaque and dull, like nitrate of lead or of barytes.

ſlightly ſol. in alcohol.

It is ſomewhat ſoluble in alcohol. Muriatic acid decompoſes it; as does nitric, and even acetous acid: but the reſult of this decompoſition is not, as might be expected, nitrate

or

or acetite of silver. At the moment that the acid is expelled from hyperoxigenized muriate of silver, a re-action takes place among its elements: oxigen is disengaged; and the muriatic acid remains in combination with the oxide of silver. If this fact be compared with the manner in which nitric and acetous acids act upon hyperoxigenized muriate of potash, it will give a strong proof of the proportionate affinities of all these acids for oxide of silver, in comparison with that which they exercise towards the alkali.

Acids decompose it, but leave muriate instead of seizing the silver.

Hyperoxigenized muriate of silver, when exposed to a very moderate heat, begins by melting, and then gives out a considerable quantity of oxigen gas, with effervescence; and muriate of silver remains behind. These phenomena however differ much, according to the degree of heat applied. When hyperoxigenized muriate of silver is mixed with about half its weight of sulphur, it detonates in the most violent manner; and does not, like hyperoxigenized muriate of potash, require the addition of charcoal, to possess a very great force of explosion. The slightest pressure is sufficient to cause this mixture to detonate; and I think I shall be within bounds, when I state, that half a grain of hyperoxigenized muriate of silver, with a quarter of a grain of sulphur, explodes with a violence at least equal to five grains of hyperoxigenized muriate of potash, with the due quantities of sulphur and charcoal. The flash is white and vivid, and is accompanied by a sharp and quick noise, like the fulminating silver so ably described by Mr. Howard; and the silver is reduced to the metallic state, and vaporized.

Hyp. ox. m. of silver loses oxigen by heat, and becomes muriate.

With sulphur it explodes; by the slightest pressure, at least ten times as strongly as hyp. ox. mur. of potash.

I think it right to add a few remarks upon what I have termed the proportionate affinities of acids and of bases, one for the other. It is a law, not indeed universally, but frequently observed, and very well worthy of consideration, that the acids are attracted by metallic oxides, in a very different order from that in which they are disposed to unite to alkaline and earthy bases.

Order of attraction of acids for oxides is frequently very different from that with alkalis.

Nitric acid, which holds so high a place in the order of affinities for alkalis, is expelled from metallic oxides by most acids. Phosphoric, fluoric, all the vegetable acids, except two or three, and the animal acids, attract the latter bases more strongly. Nay, we shall find, upon an attentive examination, that

Acids commonly attract oxides more the less they act on the metals:

that acids commonly attract metallic oxides, in the inverse ratio of their action upon metals, or, in other words, in proportion to their own affinity of composition. Thus, the phosphoric and fluoric acids sometimes rank before the sulphuric; and the nitric, as I before said, is generally very low. Hyperoxigenized muriatic acid seems to follow the same rule; and takes its place, in the order of affinities for metallic oxides, after many of those acids which it can expel from earths and alkalis.

Hyp. ox. m. acid follows this rule.

The metallic butters have their metal and not their acid extraordinarily oxigenized.

The other hyperoxigenized muriates, I have not yet sufficiently examined. I shall, however, mention at present, that I have ascertained the muriatic salts, formerly known by the strange name of *butters of the metals*, to be muriates, and not hyperoxigenized muriates; and the extraordinary proportion of oxigen, to be combined, not in the acid, but in the metallic oxide.

In the course of different experiments, I have known hyperoxigenized muriatic acid to be formed in two cases, where I could not have expected it.

Precipitation of oxide of titanium from mur. acid by potash, gave oxigen to the latter, and formed ox. m. potash.

In the analysis of some menachanite from Botany Bay, given to me last year by the President of the Royal Society, I observed, that while the oxide of titanium was precipitated from the muriatic acid in which it was dissolved, the excess of oxigen in the oxide passed over to the muriatic acid and the potash, already in the liquor, and that hyperoxigenized muriate of potash was formed. I have attempted the same experiment with black oxide of manganese, but could not succeed.

When nitro-muriatic acid is distilled from platina, *the metal is oxided, and ox. and hyper-ox. m. acid are formed.*

There is, however, a still more extraordinary formation of this acid, in the distillation of nitro-muriatic acid upon platina. Oxigen is absorbed by the metal; yet, not only oxigenized, but also hyperoxigenized muriatic acid is formed. I have repeated the experiment several times; and am well convinced of the fact, however contrary to theory it may appear. I have tried the action of oxigenized muriatic acid, upon nitric acid, in the hopes of forming hyperoxigenized muriatic acid; but there was no action to this effect among their elements.

This fact imperfectly seen before.

The fact of the production of a peculiar gas, by the distillation of nitro-muriatic acid upon platina, has been observed by Mr.

Mr. Davy, in his *Refearches* *. But, as hyperoxigenized muriatic acid was not known at that time, he could not fay the real nature of that gas. Had Mr. Davy carried his ingenious experiments a little farther, we fhould have been much earlier acquainted with the laft degree of oxigenizement of muriatic acid.

Mr. Berthollet terminates his Paper upon hyperoxigenized muriate of potafh, by faying, that he will confider muriatic acid as the radical; oxigenized muriatic acid, as correfponding with fulphureous and nitrous acid; and the acid which he conjectured to exift in this falt, as correfponding with fulphuric and nitric acid. I fhall now conclude, by ftating the arguments in favour of each denomination, and the analogies upon which they are founded.

Berthollet confiders muriatic acid as a radical, &c.

Muriatic acid is for us a fimple body; but it has acid properties of the ftrongeft kind; therefore, from analogy, we fuppofe it to contain oxigen. But may not this be too hafty a conclufion? Are we not very doubtful concerning the exiftence of oxigen in pruffic acid? And are we not, on the contrary, certain that fulphurated hidrogen, which poffeffes many of the characteriftics of acids, does not contain any? Of the oxigenizement of fluoric and boracic acids, we have no proof: but then we cannot affirm that any one of thefe acids exifts in three ftates of combination with oxigen; and the muriatic is the only radical of which we admit this fact. We muft not, however, pretend to limit the number or degrees of combinations between combuftible bodies and oxigen; but we can fpeak with certainty only of thofe things which are proved. Befides its acid properties, this fubftance has others, common to oxigenizable bodies. With 16 of oxigen, it forms an acid, which, in many of its properties, is to its radical what the fulphureous is to fulphur. Like the fulphureous, it is volatile; has little attraction for falifiable bafes; deftroys vegetable blues; and is capable of further oxigenizement. With 65 of oxigen, it becomes more fixed, like fulphuric acid; has a ftronger affinity for falifiable bafes; and acquires more truly acid properties. Upon thefe confiderations, I fubmit to

The arguments ftated on this fubject.

Mur. acid, like fome others, has the properties of oxigenizable bodies, and may not contain oxigen.

* Dr. Prieftley, alfo, mentions a peculiar gas, produced by diftilling a folution of gold in *aqua regia*.

the

the chemical world, whether, in the present ſtate of our knowledge, it be not more philoſophical to ſay,

Nomenclature of this hypotheſis.

Muriatic radical, or ſome ſingle word of the ſame import;	instead of	Muriatic acid;
Muriatous acid,		Oxigenized muriatic acid;
Muriatic acid,		Hyperoxigenized muriatic acid,

It is not proved that every acid contains oxigen.

I am fully aware that, at firſt ſight, this may appear extraordinary; and the more ſo, as we have no poſitive facts that prove muriatic acid to be a ſimple body. All we can, therefore, conſider fairly, is, in favour of which appellation does the ſum of analogies ſeem to preponderate. And, to give the cauſe a candid inveſtigation, we ſhould begin by conſidering, whether the preſence of oxigen in all bodies that have acid properties, has been rigidly demonſtrated; and not determine by this law of the French chemiſtry, till we are well convinced it has not been too generally aſſumed.

Nomenclature ſhould be governed by the facts; and not the contrary.

If a nomenclature be not ſubſervient to the uſes of ſcience, and does not keep pace with its progreſs, the relation between ſubſtances and their names will become ſo relaxed, that confuſion will be brought about, by the very means we take to avoid it; and if, while we continue to extend our acquaintance with chemical bodies, nomenclature remains confined within its former limits, the bonds that unite theſe two parts of the ſcience muſt inevitably be broken.

III.

On the preſent State of the Britiſh Muſeum. By a Correſpondent.*

The Britiſh Muſeum ſtated to be of no public utility.

IT will probably be conſidered a preſumption, even bordering on impertinence, to remark, that no eſtabliſhment is of leſs utility to the public, and none a greater reproach to the nation, than the Britiſh Muſeum in its preſent ſtate; but as the remark is made with the pureſt motive, it is hoped its apparent petulance will be excuſed.

That it is not open to the public in any beneficial manner;

That this magnificent and valuable collection, bought by the public for their own uſe, and entertained at their own expence,

* Whoſe name accompanied the paper.—W. N.

pence, ſhould be rigorouſly ſhut up from the public themſelves, is ſo extraordinary and diſgraceful a fact, that I am wholly at a loſs to account for it, particularly when I reflect that the fact occurs in a nation who prides itſelf, above all things, on the free enjoyment of its liberty and its property.

It may be objected to me, that the Muſeum is not literally ſhut up from the public, and that, on the contrary, it is in the power of any individual to procure a ſight of it. True; it is in the power of any one to procure a curſory view of it, after the formality of inſcribing his name on the regiſter of admiſſion, and waiting until the keepers think proper to appoint him a preciſe day and hour, when it is very likely his other engagements may deprive him of the poſſibility of attending. But, ſuppoſe the appointment procured, and the time convenient, what gratification or inſtruction can be derived from prowling through a ſuite of apartments, at the heels of a keeper, who is anxious to diſpatch you as ſoon as poſſible, and in company of a dozen people, each of whom is eager to catch a tranſient glance of every object within his reach, and each exclaiming at the "wondrous ſurpriſing" things which ſurround him.

because the times of inſpection are not made convenient to ſtudents;

and the actual proceſs is that of a multitude hurried through a ſuite of rooms.

The library, the moſt valuable and moſt extenſively uſeful part of the Britiſh Muſeum, is preciſely that part which is moſt completely ſecluded from the public. To viſit this it is indiſpenſably neceſſary to be introduced to the librarian, and to obtain *his* permiſſion to conſult the collection. How different is it on the Continent! there, ſcarce a town of any note but has its public library, to which free acceſs may be had at all times, by every individual, whether ſtranger or native. How humiliating the compariſon! and how ſingular that London, where the ſciences and the arts are cultivated with greater ſucceſs probably than in any other ſpot of the globe, ſhould be deſtitute of a ſingle public library.

The library *ſaid to be* acceſſible only at the pleaſure of the librarian.

Not ſo on the Continent.

Reflection.

If I might preſume to ſuggeſt a plan for rendering the Britiſh Muſeum a real benefit to the public, I would propoſe that the library, and the collection of objects relative to natural hiſtory, &c. ſhould be formed into two ſeparate eſtabliſhments, to each of which the public might have the freeſt acceſs during as great a portion of the day as poſſible.

Suggeſtion; that the library and collection ſhould be ſeparated.

The library might be open all the year round, from eight in the morning until four in the afternoon, and again from ſix until ten at night. It ſhould be furniſhed with tables, pens,

Regulations for the library.

3 and

and ink: the floor ſhould be matted, and the room properly heated during the winter ſeaſon. The books ſhould be delivered by the aſſiſtant librarians to thoſe who might require them; but on no account ſhould any book be permitted to be taken out of the library. A regular regiſter might be kept of the books delivered out each day, with the names of thoſe who required them; and the delivery might be cancelled as they were returned to the librarian. It might perhaps be proper, on account of the value of the manuſcripts, to require the expreſs permiſſion of the principal librarian for their peruſal.

Concerning the expences and other objects.

A proper fund ſhould be provided for the neceſſary expence of the library, which might be under the direction of a committee: ſuch ſums as were over and above the expence of the maintenance of the library, might be applied to the purchaſe of ſuch books as the committee ſhould ſee expedient. But to contract, as far as poſſible, the expence of ſupplying the library with new publications, the donations of public ſpirited individuals ſhould be ſolicited, and the names of the donors regiſtered in a conſpicuous manner on each book they preſented. The committee might be empowered to ſell, by public auction, from time to time, ſuch duplicates as might be received, &c. and be thought unneceſſary to retain in the library. The accounts of the library might be adjuſted every quarter; but they ſhould conſtantly remain open for the inſpection of the public. A regular catalogue of the library ſhould be printed in octavo; and ſupplements might be added from time to time, as the new acquiſitions ſhould render it neceſſary: one or more of theſe catalogues ſhould be conſtantly kept in the library, for the purpoſe of general reference. A binder and proper aſſiſtants ſhould be attached to the library, through whoſe hands each volume ſhould paſs at leaſt once every year; and it might not be beneath the attention of the committee, to hold out premiums for any improvements which ſhould be made in the binding of books, ſo as more effectually to preſerve them from the attack of the worm, and from gradual decay.

The collection of natural hiſtory. Direction.

The collection of objects relative to natural hiſtory, &c. ſhould be under the ſuperintendance of a ſeparate committee; and this eſtabliſhment, if properly conducted, could hardly fail to become one of the fineſt and moſt inſtructive repoſitories in Europe. No nation is ſo well ſituated for eſtabliſhing a national Muſeum as our own; our extenſive commerce, and the

the public ſpirit of individuals, would ſhortly ſupply us with the choiceſt ſpecimens from every quarter of the globe; and at this very moment thouſands of valuable ſpecimens are rotting in obſcurity, for want of a national Muſeum to receive them.

Arrangement of the collection.

The arrangement of this eſtabliſhment requires conſiderable knowledge and ability; it would therefore be the higheſt preſumption in me to attempt to dictate on the ſubject. I ſhould conceive, however, that the whole collection might be advantageouſly ſeparated into five claſſes. The firſt might comprehend the animal, the ſecond the vegetable, and the third the mineral kingdom; the fourth might comprehend objects of pure curioſity, and ſuch as were illuſtrative of the ſtate of ſociety, of arts, and of manufactures of various nations; the fifth might compriſe ſuch objects as tended to the improvement of, or were ſimply illuſtrative of the arts, ſciences, and manufactures of our own country.

Five claſſes.

Animals; vegetables; minerals; curioſities; objects of art.

Natural hiſtory.

Subjects.

In the arrangement of the objects of natural hiſtory, I would ſuggeſt that the moſt extenſive ſcale ſhould be aſſumed, and that ſpaces ſhould be left for the inſertion of ſuch objects as were wanting, and were likely to be procured: the places of theſe objects ſhould be ſupplied by accurate drawings or models, if poſſible. To each object might be attached its proper name, both in Latin and in Engliſh; and a deſcriptive catalogue of the Muſeum ſhould be printed, which might at the ſame time ſerve as an introduction to the ſtudy of each claſs.

Catalogue.

Donations.

The donations of public ſpirited individuals ſhould be ſolicited for this eſtabliſhment, as for the library; and the names of the donors ſhould be regularly inſerted in the public prints. The committee ſhould alſo be empowered to diſpoſe of ſuch duplicate ſpecimens as might be ſent, by public auction.

Laboratory.

A laboratory for the preparation of objects of natural hiſtory ſhould be annexed to the eſtabliſhment; and the committee might be directed to offer premiums for improvements in the art of preparing and preſerving theſe objects. The committee might alſo be enjoined to publiſh conciſe directions for travellers, for collecting, preſerving, and tranſporting the different objects they might meet with; copies of this work ſhould be given to all who might requeſt them.

Conſequences of ſuch an extenſive and liberal

I am inclined to think that if this eſtabliſhment was conducted on an extenſive and a liberal ſcale, it would ſhortly compriſe

moſt

conduct of this establishment.

most of the private collections in the kingdom; for it is presumed that there are few lovers of natural history who would not prefer the honourable applause of completing the national Museum, to the selfish pleasure of possessing an imperfect and comparatively useless collection of their own, especially as they would enjoy the credit of the collection equally as if it remained in their own private possession.

Extension of the Museum.

By degrees as the collection enlarged, and as its utility was more sensibly felt, it is to be presumed that the nation might be induced to purchase ground, and to erect appropriate and extensive buildings to contain it: in short it would seem, were this plan to be adopted, that we might indulge ourselves with the prospect of speedily possessing one of the finest, the most interesting, and most instructive establishments in the universe.

J. G.

ANNOTATION. W. N.

Public establishments are of great importance.

They have vigour at first;

Though the importance and value of public establishments have been strongly felt and admitted in all ages, yet there has not, as far as I know, been any direct investigation of the means of giving them permanence and effect. A public library, college, institution, or society, may be radically defective in its constitution, and, nevertheless, flourish for a time by virtue of the ability and active exertions of its founders; and it is certain, that every constitution whatever will afford only a perishable establishment, unless it contain some inducement by which such exertions shall be invited and continued in its favour. It appears to be extremely difficult, and perhaps in strictness impossible, to produce motives of this kind by mere regulation; and trustees, managers, inspectors, or governors, being bound by the letter of the statutes, can seldom be expected to make advances in support of the spirit of the foundation. Is it an unavoidable consequence, that schools, universities, and societies, shall degenerate into mere instruments for conferring degrees or titles? Are all establishments perishable unless endowed? Is it from the operation of inevitable causes that various endowments are productive of little or no effect, and that most unendowed establishments disappear in a few years? Among the former we may mention the Gresham foundations and

but soon decay.

Is this degeneracy inevitable?

Endowments, &c.

Instances. Gresham College and others.

and various public libraries in this town, with ſeveral colleges, ſchools, and ſocieties, which it would anſwer no good purpoſe to name without more ample comment; and among the latter the colleges of Warrington and Hackney, begun with great ſpirit, but in a few years abandoned. Or may we not rather hope, that an inveſtigation of conſtitutional and hiſtorical facts, reſpecting all the public means of inſtruction in this and the neighbouring countries, would aſſiſt us in the means of giving full energy to theſe important eſtabliſhments? How are the collections and libraries on the Continent regulated as to ſupply of materials, preſervation from injury or embezzlement, and ready acceſs by the ſtudent? What may be the ſtate of the muſeums and libraries in London, and our univerſities, in theſe and other reſpects? Can the collective power of any body of men be eminently applied to forward literature and the ſciences; and if ſo, why has it generally failed? &c. Theſe and many other intereſting ſubjects of inquiry, might be cleared up by Correſpondents of Journals, who may not have leiſure for the direct and ample diſcuſſion of a ſubject of ſuch magnitude and extent.

The hiſtory of theſe objects may probably ſhew how to preſerve their energy. Correſpondents invited to ſtate the orders of the continental libraries, the Britiſh collections, &c. in order to make progreſs.

IV.

Of the Properties of the Earth Yttria, *compared with thoſe of* Glucine; *of Foſſils, in which the firſt of theſe Earths is contained; and of the Diſcovery of a new Subſtance of a metallic Nature (Tantalium). By* A. G. EKEBERG *.

THE firſt part of this Memoir contains the particulars of the experiments made by M. Ekeberg, to confirm the difference exiſting between glucine and the earth laſt diſcovered in gadolinite, and named yttria or gadoline; but as the particular nature of each of theſe earths is well known to chemiſts, it would be ſuperfluous to ſay any thing in this place on the ſubject.

First part of the Memoir.

* *Kongl. ſwenſk. Vetenſkaps, academiens nya Handlingar; fser aer* 1802, *Qvartal I. Pag.* 68.

This abridged Memoir is from the Journal de Van Mons, but I take it from the Journal de Phyſique, LV. 281.

As

As there is novelty to recommend the ſecond part of this Memoir, we ſhall communicate it to our readers in a faithful tranſlation.

Mineral ſubſtance newly inveſtigated, containing a metal hitherto unknown.

The author remarks, that although the mineral ſubſtance which he has diſcovered contains yttria, it cannot be claſſed in a ſyſtem of mineralogy as a ſpecies of that earth, on account of the more abundant preſence of another equally remarkable ſubſtance, which muſt be carried to the numerous claſs of metals. He found this ſubſtance in two foſſils obtained from different ſituations: in one place it was united with iron and mangaueſe, and in the other with iron and gadoline.

Inſoluble in all the acids; ſoluble by fuſion in alkali; precipitable by acid, but not ſoluble; the precipitate white, even after ignition; ſoluble by heat with alkaline phoſphate or borate; but gives no colour.

This new metallic ſubſtance is characterized by its inſolubility in all the acids. The only re-agent that has any action on it, is fixed cauſtic alkali. When expoſed to the fire with that alkali, the maſs being afterwards lixiviated, it partly diſſolves in water, and may be precipitated from this ſolution by means of an acid, but the precipitate is not in the leaſt attacked, however great the quantity of acid employed. When ſeparated by filtration and dried, it has the form of an extremely white powder, which does not change colour even at a red heat. The remainder of the maſs being treated with acids, affords the ſame powder. Its ſpecific gravity, after ignition, is 6,500. It is fuſible with the blow-pipe, by the addition of alkaline phoſphate and borate of ſoda, but it does not communicate any colour to the flux.

It is reducible to a metallic button by charcoal; and is then oxidable, but not ſoluble by acids.

When expoſed to a ſtrong heat in a crucible, without any mixture except pounded charcoal, it is reduced to a moderately hard button, having a metallic brilliancy at its ſurface, but its fracture is dull and blackiſh. The acids have no other action on this kind of regulus, than that of cauſing it to return to the ſtate of white oxide. The circumſtances of the reduction, as well as the ſpecific gravity of this ſingular ſubſtance, ſeem to place it among the metals, and I have ſufficient reaſon to be perſuaded that it is not the ſame as any of thoſe hitherto known.

It appears to be a new metal; not tin, becauſe inſoluble, and not, like it, reducible; nor tungſten, becauſe it is nei-

The ſubſtances with which it is likely to be confounded are the oxides of tin, of tungſten, and of titanium, which are ſoluble in the cauſtic alkalis, and in all circumſtances reſiſt the action of acids. But the oxide of tin is eaſy to be diſſolved and reduced; tungſten is eaſily detected by its ſolubility in ammonia, and by the blue colour it communicates to

to phoſphate of ſoda; the oxide of titanium gives to borax the colour of hyacinth, and becomes ſoluble in acids when fuſed with carbonate of potaſh *.

ther ſoluble in ammonia, or gives a blue colour to phoſ. ſoda; nor titanium, becauſe it does not colour borax, nor become ſoluble in acids when fuſed with carb. potaſh.

Before the author deſcribes the chemical analyſis he has undertaken of theſe two ſubſtances, which he regards as ores of the new metal, he has choſen to deſcribe their external characters. In order to avoid circumlocution, he has ventured to give them a generic name. By availing himſelf of the uſage which admits of mythological denominations, and in order to expreſs the want of power of this new metal to become ſaturated with the acids into which it is plunged, he has given it the name of *tantalium;* for the ore compoſed of tantalium, of iron, and of manganeſe, he propoſes the name of *tantalite;* and for that containing yttria, the appellation of *yttrotantalite,* which cannot be thought more heavy than that of ſiderotitanium, already adopted.

External characters.

The new metal denominated tantalium.

its ores tantalite and yttrotantalite.

He received the ſpecimen of tantalite from M. Geyer, Director of Mines, who has aſſured him that this ſubſtance has been known ſince the year 1746, and has been regarded as a problematical variety of tin garnet *(zinngraupen)*. It is found near the farm Brokaern, in the pariſh of Rimito, in the government of Abo, in Finland, in a large mountain, on the banks of the Baltic. The gangue is compoſed of white quartz and mica, with veins of red feld ſpar in large plates; ſubſtances which compoſe the ſides of the gangue. The titanite is diſſeminated in the form of garnets.

Locality and gangue, &c. of tantalite.

The ſpecimens he has ſeen were in detached cryſtals, of the ſize of a nut, the moſt regular among which ſeemed to approach the octahedral form: they contained particles of feld ſpar and mica.

Size and figure of cryſtals.

Their ſurface is even, poliſhed, and blackiſh.

Surface.

Fracture compact, and of a metallic brilliancy; colour of the fracture not alike all through; it varies between a greyiſh blue and the black of iron.

Fracture.

When pulverized, it is of a blackiſh grey approaching to brown.

Powder; grey browniſh.

* The author nevertheleſs entertained ſome ſuſpicions of the identity of this new ſubſtance with the latter metal, and nothing but a compariſon with the titaneous iron of Norway, which he decompoſed with this view, convinced him that this identity does not exiſt.

It

Gives sparks. It is sufficiently hard to give sparks with the steel.

He did not remark whether it is attracted by the magnet.

Sp. gravity. Its specific gravity is 7,953.

Locality and gangue, &c. of yttrotantalite. The yttrotantalite is found in the same place and in the same gangue as gadolinite. Klaproth says, that the latter is found incrusted in a mass of granite; but though the constituent parts of granite are occasionally found with it, it is not the less insisted that feldspar is its true gangue, as is clearly seen in the instances of the great quarry at Ytterby. The mica and quartz found in it form distinct parts, and do not enter into combination with the feldspar; (he speaks only of the masses of the principal rock, without disputing the possibility of particles of the three substances being found mixed); but in general it is a rock of feldspar intersected by large veins of mica in a direction almost perpendicular; and the gadolinite and yttrotantalite are to be sought near these veins. The first is commonly found attached to one side of a vein of silvery mica, and the rest of its mass enveloped with feldspar. The second is never found immediately adherent to mica. The masses or kidnies which it forms, are enveloped with a thin crust of feldspar, separated from the general mass by slight layers of a greyish black mica. The gangues charged with these kidnies are seldom insulated: They are found in clusters, separated from the principal rock and from one another by similar coatings of mica. These substances are generally thus disposed by nature, and it is very seldom that they are found disseminated in grains in the rock of feldspar.

Size and fracture of the masses. The largest masses he has found of the ore of yttrotantalium were not of the size of a nut. Its fracture is granulated, of the black colour of iron, with a metallic brilliancy.

Hardness. Its hardness is not considerable; it may be scraped with a knife, though difficultly.

Powder. When in powder it is of a greyish colour.

Not magnetical. The magnet does not attract it.

Sp. gr. Its specific gravity was found to be 5,130; but as no specimen was found totally free from feldspar, it was suspected that its real weight is more considerable.

On gadolinite. The abridger concludes his extract by giving an outline of that part of M. Ekeberg's Memoir, which treats of the analysis of gadolinite and the comparison of yttria with glucine, which several writers have affirmed to be of the same nature.

Gadolinite

Gadolinite when pure is hard enough to give fire with ſteel. It is hard;

It is found cryſtallized in an imperfect manner like ſome ſpecies of garnets. cryſtallized;

It contains glucine. contains glucine.

Beſides the diſtinctions between gadoline and glucine eſtabliſhed by Klaproth and Vauquelin, theſe earths differ in the following peculiar properties. Gadoline and glucine differ

The ſpecific gravity of gadoline differs conſiderably from that of glucine, which is only 2,967, whilſt that of gadoline is 4,842. This laſt earth is the heavieſt of all the known earthy ſubſtances, ſince it is 0,842 heavier than barites, which only weighs 4,000. in ſpec. gravity.

It is ſoluble in the alkaline carbonates. Gadoline is ſoluble in alkaline carbonates.

It is abſolutely inſoluble in the cauſtic alkalis. not in pure alk.

It is not precipitated by the ſuccinates as glucine is. nor (like glucine) precip. by ſuccinates.

V.

On the Probability that the Eruptions of Lunar Volcanos may reach the Earth. By J. B.

To Mr. NICHOLSON.

SIR,

Cambridge, Nov. 6, 1802.

THE fact of ſtony and metalline ſubſtances having at different times fallen on the earth, ſeems to be confirmed by ſuch ſtrong evidence, that a doubt can ſcarcely remain as to its reality; permit me through the medium of your Journal, to offer to your readers an attempt to account for this phenomenon. The exiſtence of lunar volcanos, at leaſt equal in magnitude to thoſe of the earth, has been proved by ocular demonſtration. The velocity with which a body muſt be projected from the moon's ſurface ſo as to be carried within the ſphere of the earth's attraction, is about 12,000 feet per ſecond; now when we conſider the height to which volcanic ſubſtances are projected during an eruption, and how very conſiderable a part of ſwift motion muſt neceſſarily have been deſtroyed by the reſiſtance of the atmoſphere, I think it by no means improbable, that the force exerted may be conſiderably greater than would be neceſſary to carry a body 12,000 feet *in vacuo,*

Stones fallen on the earth. Lunar volcanos. Velocity of a projectile to be thrown from the moon.

The lunar atmosphere resists little.

vacuo. The atmosphere of the moon cannot offer so great resistance to the motion of bodies as that of the earth, as it is certainly much rarer than ours, and probably its height is not much greater than that of the lunar mountains. From all these circumstances taken together, I think it by no means improbable that the volcanoes of the moon may occasionally project bodies with sufficient force to carry them to the earth.

Whence bodies may be thrown to the earth.

I remain, SIR,
your's,
J. B.

OBSERVATIONS.—W. N.

Remarks on the above letter;

Against the conjecture of J. B. it seems to be a principal difficulty that the stones fallen on the earth are thought to have arrived in a state of ignition, which they must have preserved through their immense passage from the moon. If the meteors or ignited globes be allowed to be stones of the kind in question, their extreme velocity in some instances may be thought to favor his hypothesis, But this subject, like every other in which the data are few, affords wide range for speculation. When we reason *from our want of knowledge*, there is scarcely any branch of science more favorable to the process than chemistry. We are ignorant of the component parts of the earths, the metals, oxigen, azote, hidrogen, &c. and of the very existence of light, caloric, electricity, and magnetism, as independent bodies. Some, if not all of the former may be, and most probably are, compounded. We see very sudden or instantaneous separations of water, light and electricity from the atmosphere. May not iron and nickel be among its ingredients, and may they not by causes equally unknown, be no less rapidly separated, with the emission of light and heat? Even if these metals exist in the atmosphere disseminated, suspended, or dissolved like aqueous vapors, (and we know they emit effluvia perceptible to the smell) their quantity may elude all chemical tests from its minuteness, and still be sufficient to answer the phenomena. A cubic foot of air does not weigh 100 grains, and if one thousandth part of this were metal, the quantity contained in the air to the height of 500 feet over a single field of ten acres would be about a ton and a half, of which a small part being precipitated, would afford a prodigious shower of stones.

with other very loose conjectures.

VI. *Enquiries*

VI.

Enquiries concerning the Dilatation of the Gases and Vapors. Read to the National Institute of France. By CIT. GAY LUSSAC.

(*Concluded from Page* 216.)

THE following is the table of results of Citizen Guyton and Duvernois, in which they have placed between brackets those in which they place little confidence.

	From 0° to 20°	From 20° to 40°	From 40° to 60°	From 60° to 80°	From 0° to 80°
Common air dilates to	$\frac{1}{12,67}$	$\frac{1}{5,61}$	$\frac{1}{2,49}$	$\left(\frac{1}{3,57}\right)$	$\frac{1}{1,067}$
Vital air	$\frac{1}{22,12}$	$\frac{1}{4,92}$	$\frac{1}{1,53}$	$\left(3+\frac{1}{1,73}\right)$	$4+\frac{1}{2,09}$
Azote gas	$\frac{1}{29,41}$	$\frac{1}{5,41}$	$\frac{1}{1,82}$	$5+\frac{1}{57,2}$	$5+\frac{1}{1,062}$
Hidrogen gas	$\frac{1}{11,91}$	$\frac{1}{6,92}$	$\left(\frac{1}{6,85}\right)$	$\left(\frac{1}{58,82}\right)$	$\frac{1}{2,55}$
Nitrous gas	$\frac{1}{15,33}$	$\frac{1}{9,00}$	$\frac{1}{3,739}$	$\left(\frac{1}{6,88}\right)$	$\frac{1}{1,65}$
Carbonic acid gas	$\frac{1}{9,049}$	$\frac{1}{5,099}$	$\frac{1}{2,31}$	$\left(\frac{1}{3,69}\right)$	$1+\frac{1}{106,3}$
Ammoniacal gas	$\frac{1}{3,58}$	$\frac{1}{1,75}$	$1+\frac{1}{1,35}$	$\left(3+\frac{1}{4,69}\right)$	$5+\frac{1}{1,248}$

Before I proceed further, I ought to premise, that though I had frequently found that oxigen, azote, hidrogen, carbonic acid gas, and atmospheric air, dilate equally from 0° to 80°, Cit. Charles had remarked the same properties in these gases fifteen years ago; but having never published his results, it was by mere chance that I became acquainted with them. He likewise endeavoured to determine the dilatation of gases

which are ſoluble in water, and he found in each a particula dilatation differing from thoſe of the other gaſes. In this reſpect my experiments differ much from his.

Apparatus of Cit. Charles. A barometer with a large vacuum, and a reſervoir below for the gas to preſs on the mercury.

The apparatus of Cit. Charles conſiſted of a barometer whoſe chamber or upper ſpace was of great extent. The gas on which the experiment was to be performed was encloſed in the reſervoir, or lower ſpace of the barometer, at the temperature 0°, and under the preſſure of 28 inches of mercury. When the barometer was plunged in boiling water, the mercury roſe in the tube, and the exceſs of the whole column above 28 inches indicates the elaſticity acquired by the gas;

This was inaccurate, becauſe the riſe of the mercury gave room for expanſion.

but Cit. Charles having had the politeneſs to ſhew me his apparatus, I ſaw that the tube of the barometer was too large in proportion to the capacity of the reſervoir; ſo that the elevation of the mercury above 28 inches did not indicate all the ſpring the gas had acquired, becauſe in that caſe its volume in the reſervoir ought to have remained conſtant. It appears then to me, that the real dilatation of the gaſes cannot be decided from his experiments.

Art. III. *Deſcription of Apparatus.*

Apparatus for the gaſes. A globe with an iron cock; filled as uſual, and immerſed in water to be heated. The gas is ſuffered to expand and paſs out, and the expanſion meaſured.

A receiver B (Fig. 1. Pl. IX.) has a cock of iron R, to which may be adjuſted a bended tube I, D, (Fig. 2). The key of the cock carries a lever L L bored at its two extremities in order to receive two cords, by means of which the cock may be opened and ſhut under the water. In order to introduce the gas into the receiver I uſe a bell glaſs M, (Fig. 1.) to which are adapted a cock and bended tube T, plunged in a veſſel Q, S. When water is poured into the veſſel, and the cock opened, the gas compreſſed in the bell eſcapes through the tube and fills the receiver B, reverſed on a bath P, O, of mercury. The receiver B being full I ſhut the cock: I adjuſt the tube I, D, (Fig. 2.) to it, and I fix it in a cylindrical cage of iron E, F, G, H, which I then carry to a copper veſſel A, D, filled with water. In order that there might be no communication between the exterior air and the gas incloſed in the receiver, when the cock is opened I cauſe the extremities of the tube I, D, of one or two millimetres (thirtieths of an inch) to be plunged in a ſmall bath K, X, of mercury. This done I heat the bath, and from 10° to 10° for example, I open the cock and ſhut it immediately. The gas dilated by the

the heat coming briſkly out of the receiver, quickly drove off the atmoſpheric air that filled the tube, and after the 40° the cock may be ſafely left open during the remainder of the operation: I prefer however alternately opening and ſhutting it, becauſe I find that the gas of the receiver acquires the temperature of the bath better. After 15 or 20 minutes of ebullition, a time ſufficient for the whole to acquire the ſame temperature, I diſengage the extremity of the tube I, D, from the mercury in order to re-eſtabliſh the equilibrium of preſſure between the exterior air and the gas of the receiver, and I then cloſe the cock. After having cooled the bath with ice or water I take away the apparatus, I diſengage the receiver from which I take the tube I, D, and the lever L, L, and I plunge it altogether in a bath of a known temperature, where I leave it a ſufficient time to acquire its temperature.

When the cock is opened a volume of water enters the receiver, which when reduced to a level is preciſely equal to that of the gas expelled by the heat. When the cock is ſhut I withdraw the receiver; I dry its exterior ſurface with care, and I weigh it in this ſtate. I then weigh it firſt full of water, and then empty, and note the reſults of each weight. By this knowledge I obtain the capacity of the receiver by ſubtracting its weight when empty, from its weight when filled with water; and alſo the volume of the water which repreſents the volume of air expelled by the heat from the receiver, by ſubtracting again the weight of the receiver when empty, from its weight when containing this water. Hence it is very eaſy to determine the proportion of the firſt volume of the air to that of the dilated.

The relative bulks of the gas at different temperatures determined by weighing.

This method has the advantage of being very exact; for as the volumes are determined from the weights, any error that is committed in this determination muſt be very trifling, even if the ſcales employed ſhould be but moderately ſenſible.

This method is very exact.

The apparatus I have juſt deſcribed is ſimple enough in itſelf; but as it contains cement, and a cock which ought to be made of iron on account of the mercury, it is rather difficult to execute. It will not therefore be amiſs to deſcribe another apparatus which I employ, and which on account of its great ſimplicity and eaſy execution, very nearly combines all the advantages of the former.

Another apparatus nearly as exact and more eaſily conſtructed.

A body with a long neck; graduated, It is a simple globe or body, and the neck of which must be at least one decimetre (about four inches) long. After having filled it with the gas by the method already described, I plunge its neck about two centimetres (three quarters of an inch) in mercury contained in a common glass vessel, and I secure it by an iron frame like the preceding apparatus. If I were to plunge it in this state into a bath of heated water, the dilated gas in order to escape would have to overcome not only the pressure of the mercury in the glass, but likewise that of the water of the bath. To remedy this inconvenience, I introduce into the neck of the receiver the extremity of a very fine bended tube, taking the precaution to keep the upper aperture closed till the tube is plunged beneath the mercury. To support the tube I tie a string about its middle, to the end of which I suspend a weight, and I pass it over a support in such a manner, that the cord by its action shall have a tendency to draw the tube upwards. The apparatus being thus disposed I plunge it in a glass vessel, the water in which must be on a level with that in the bath: I open the extremity of the tube for an instant, that the equilibrium of pressure with the external air may be restored, and I then shut it. As there is a scale marked on the neck of the receiver whose divisions are extremely minute, I find exactly the level or station of the mercury in the neck, and I note it, because it is at this station that the capacity of the receiver terminates. The lower extremity of the tube must rise clearly above the surface of the mercury, which otherwise would flow into the tube and resist the passage of the dilated gas. After all these operations, which are longer in the description than the execution, I carry the apparatus to a bath of hot water, and I open the upper extremity of the tube, after having plunged it into a small bath of mercury, as in the preceding arrangement. When the receiver is at the temperature of boiling water I withdraw the tube, the extremity of which must be previously disengaged from the mercury, and I cool the bath. The mercury then rises into the receiver; but it is easy to substitute water as the temperature diminishes. The capacity of the receiver, and the volume of water which replaces the gas expelled by heat, is determined in the manner already described; excepting that in this determination to the weight of the empty receiver, must

Side notes: plunged in mercury; A small tube to communicate with the outer air when requisite. Observation of the mercury in the neck. When the apparatus is plunged in boiling water, the expanded gas escapes by the tube which is then withdrawn. Water is suffered to enter as it cools, and the expansion determined by weight.

must be added that of a cylinder of water terminated at one end by the line of the station of the mercury, and at the other by the neck of the receiver.

I might add a few more details, but I shall suppress them for the sake of brevity, and because persons very little accustomed to manipulation will easily supply the defect.

However, as it is important, from what I have already said to exclude water entirely from the apparatus, I shall mention how I succeeded in completely effecting this purpose.

If the receiver was visibly moist, I first dried it with blotting paper, and then heated it so as to evaporate part of the water it might still contain, and by means of bellows, to which I adjusted a glass tube, I expelled the vapour by a current of air. These last operations being repeated several times on the receiver and tube, both were found perfectly dry. The mercury which I used in my experiments was always employed very dry and pure.

Methods of excluding moisture from the gas.

In all the experiments, the results of which I am about to give, I always reduced the gases (the dilatation of which could be determined by the apparatus here described) to the temperature of melting ice. For this purpose I had a bath with ice in it, into which the receiver was wholly immersed, after having withdrawn it from the bath where it was placed for the experiment, and was left there for about half an hour, during which I often stirred the ice. The other fixed temperature, at which I rested for the same gases, was that of boiling water.

The terms of temperature in the following experiments were freezing and boiling water.

I made some experiments at other temperatures; but they require to be again repeated, and will besides form a part of a work I have begun on the law of the dilatation of gases and vapors. I shall therefore confine myself to the dilatation of gases at a fixed elevation of temperature, which shall be that comprised between the degree of melting ice and the boiling point. With regard to the vapors, I shall compare their dilatation with that of the gases.

Experiments at other temperatures require to be repeated.

ART. IV. *Experiments and Results.*

When I used the two apparatuses here described, but oftener the second than the first, and avoiding all the causes of uncertainty

Expansion from freezing to boiling water,

Of atmospheric air,

uncertainty that I could suspect, I obtained the following six results from six experiments on atmospheric air *.

From the temperature of melting ice to that of boiling water, equal volumes of atmospheric air represented by 100, became - - - -	137,40 137,61 137,44 137,55 137,48 137,57
† The mean result is about - - -	137,50

for each degree,

If the total augmentation of volume be divided by the number of degrees which produced it, or by 80, it will be found, by making the volume at the temperature 0 equal to unity, that the augmentation of volume for each degree is $\frac{1}{213.33}$, or $\frac{1}{266.66}$ for each degree of the centigrade thermometer.

This result does not agree with Deluc.

As Deluc obtained $\frac{1}{215}$ for his coefficient, it seems at first sight that our results are the same; but if it be observed that he began at the temperature $16^\circ\frac{3}{4}$, and that I began at $0^\circ\frac{3}{4}$ it will be seen that our results are considerably different. I shall explain this difference elsewhere, and shew that the coefficients of dilatation vary according to the temperature of commencement.

Expansion of hidrogen;

Hidrogen gas obtained from iron by weak sulphuric acid was submitted to two experiments: in one, by an elevation of temperature from the degree of melting ice to that of boiling water, 100 parts became 137,49; and in another by the same elevation of temperature 100 parts became 137,66. The mean of these two results is 137,52, which differs very little from the mean result of the dilatation of atmospheric air.

* My receiver contained about 350 grammes of water (a little more than 12 ounces)

† Though the difference between these results are not very considerable, I think I might have rendered them less so, if I could have kept an account of the state of the barometer at the moment of the ebullition of the water. I have, however, always had the precaution to be assured of its thermometrical state at the moment of ebullition, in which I can assert that I never found any very sensible variations. A variation of an inch of the barometer would in fact be necessary to occasion one, of a degree, in the place of the boiling point; which can seldom happen. However this may be, this mean result 137,50 must be very near the truth.

Oxigen

Oxigen gas obtained from oxigenated muriate of potash was dilated three times, and gave the following results. —oxigen,

100 parts became - - -	{	137,47
		137,54
		137,45
The mean result of which is - - -		137,48

Azote gas obtained from the decomposition of ammonia by oxigenated muriatic acid gave the following mean results. —azote.

100 parts became - - -	{	137,42
		137,56
		137,50
		137,46
		137,55
The mean result is - - - -		137,49

By connecting the preceding results, and by comparing the dilatation of oxigen, hidrogen, and azote gases with that of atmospheric air, we obtain the following table: Tabulated results of four gases shew the same expansion

From the temperature of melting ice to that of boiling water, 100 parts of	Increase their volume to	Differences
	Parts	
Atmospheric air -	37,50	
Hidrogen gas - -	37,52	+ 0,02
Oxigen gas - -	37,48	− 0,02
Azote gas - - -	37,49	− 0,01

The trifling differences observed in the preceding results may proceed from the impossibility of rendering the circumstances rigorously the same in each experiment; and as they extend to only two ten thousandth parts of the original volume, it may be concluded with safety, that the dilatation of atmospheric air, oxigen, hidrogen, and azote gases are the same from the temperature of melting ice to that of boiling water.

In order to determine the dilatation of gases soluble in water I changed the apparatus, I used two tubes graduated at the same time on the same bath of mercury with a very small measure. Every time I used this apparatus, I took the precaution that the quantity of mercury should be the same as when the tubes were graduated. I must even observe, that if any accident happens to the vessel containing the mercury, the tubes must be again graduated on another bath. It would even

Apparatus for gases soluble in water. They were compared by exposure to heat over mercury in a tube, while atmospheric air was similarly treated.

even be proper to cut them out of the same tube of glass and to make them of equal heights, so as to render all the circumstances as similar as possible.

The tubes with the two elastic fluids were heated in a stove.

Into one of these tubes I put atmospheric air to the 100th division, for example, and in the other the gas on which the experiment was to be made likewise up to the 100th division. 100 equal measures of these gases were thus submitted to experiment. I then carried the apparatus into a stove, the temperature of which I regulated at pleasure, and I observed the course of the dilatation of the gases. Whatever attention I paid to the observation I never could perceive any difference, and I always remarked that the same divisions were passed through in the same time.

The gases were purified and dried before they were tried.

The gases thus examined were never directly received in the tubes; but they previously remained some time in an intermediate vessel into which I introduced some desiccative body, muriate of lime for example, and I then passed them into the tubes, compressing them with mercury, which I poured by means of a tube of safety adapted to the intermediate vessel. If these general precautions be neglected the dilatation will almost always be too great; because the influence of undissolved water will not be guarded against, or of some other body capable of easily taking the gaseous form.

Carbonic acid gas;

One hundred measures of carbonic acid gas obtained from marble by means of sulphuric acid, were compared with the like portion of atmospheric air. From the 5th to the 90th degree (Reaumur) the expansions were the same.

and muriatic acid gas;

One hundred measures of muriatic acid gas obtained by means of concentrated sulphuric acid from muriate of soda (very dry) being compared with 100 measures of atmospheric air from the 3d to the 86th degree, the expansions of the gases were found to be absolutely the same. This experiment, as well as the preceding, were repeated several times, and the same results were always obtained.

and sulphureous and nitrous gases expand precisely the same as atmospheric air.

Sulphureous acid gas and nitrous gas undergo the same dilatation by the action of heat as atmospheric air.

Priestley, Guyton, and Duvernois found ammoniacal gas expand more

Doctor Priestley, and Citizens Guyton and Duvernois, found that ammoniacal gas dilated very considerably. With a view to discover the cause that could have induced them to draw this conclusion from their experiments, I introduced (without preparation) into one of my tubes ammoniacal gas, obtained from

from the decompofition of muriate of ammonia by common lime, and in the other tube an equal volume of atmofpheric air. The ammoniacal gas dilated progreffively more than the atmofpheric air in proportion to the elevation of temperature; fo much fo that its volume was foon double that of the atmofpheric air, but on obferving the furface of the mercury, and the fides of the tube after the temperature was lowered, I remarked fome traces of a liquid and fome cryftalline fpecks, which could be nothing but muriate or carbonate of ammonia, and it all difappeared when the temperature was fufficiently elevated. I performed this experiment again by fuffering the ammoniacal gas to remain fome time in an intermediate veffel containing cauftic potafh, and then its dilatations from 0 to 95° exactly followed that of the atmofpheric air. In this experiment I obferved the furface of the mercury and the fides of the tubes when the temperature had returned to 0, but I could not perceive any indication of liquid, or any cryftalline fpeck. This experiment was repeated feveral times, and always with the fame event.

than common air.

becaufe water and a neutral ammoniacal falt were prefent.

Hence it may be feen that not only a liquid, but another body capable of taking the gafeous ftate, may render the experiment liable to error. Thefe caufes ought therefore to be avoided with the moft fcrupulous attention.

Water is not therefore the only caufe of error.

The experiments which I have juft related, and were all made with great care, inconteftibly prove that atmofpheric air, oxigen, hidrogen, azote, nitrous, ammoniacal, muriatic acid, fulphureous acid, and carbonic acid gafes expand equally by the fame degrees of heat; and that, confequently, their greater or lefs denfity under the fame preffure, and at the fame temperature, their more or lefs confiderable folubility in water, and their particular nature, in no refpect influence their dilatation.

Hence the different qualities of the gafes do not affect their expanfion by heat.

I therefore conclude that gafes in general, provided they be all placed in the fame circumftances, dilate equally with equal degrees of heat.

Thefe experiments on the dilatation of gafes naturally led me to examine that of vapors; but fufpecting from the preceding refults that they dilated like the gafes, I determined to make experiments on one vapor only, and I chofe in preference that of fulphuric ether, as being the moft eafy to manage.

Vapors.

In

The vapor of ether was tried in the tube; in comparifon with atmofph. air.

In order to determine the expanfion of the vapor of ether I employed the two tubes I have already mentioned, atmofpheric air being always the ftandard of comparifon. This apparatus having been kept fome time in a ftove whofe temperature was about 60°, I introduced ethereal vapor into one of the tubes, and into the other atmofpheric air, fo that they each correfponded to the fame divifion. I then elevated the temperature of the ftove from 60° to 100°, and I had the fatisfaction to find, that whether the temperature was raifed or lowered, the ether and the atmofpheric air always correfponded at the fame time to the fame divifions. This experiment, at which Citizen Berthollet was prefent, was repeated feveral times, and I never could obferve any difference in the dilatation compared with that of atmofpheric air. I remarked, however, that at fome degrees above the point of the ebullition of ether, its condenfations were a little more rapid than thofe of atmofpheric air. This is owing to a phenomenon which a great number of bodies prefent in paffing from the liquid to the folid ftate, but which does not prefent itfelf a few degrees above that of this period.

They expanded alike between 60 and 100° Reaumur.

When near its boiling point the ethereal vapor was condenfed rather more.

The conclufion may therefore be extended to vapors;

This experiment, by proving that the vapor of ether and the gafes dilate equally, fhews us that this property in no refpect depends on the peculiar nature of gafes and vapors, but only on their elaftic ftate, and we muft confequently conclude that all gafes and vapors dilate equally at the fame degrees of heat.

The vapors will be more compreffible near their point of denfe fluidity or liquefaction.

Since all the gafes are equally dilatable by heat and equally compreffible, (gas condenfable) and as thefe two properties depend on one another, as I fhall prove elfewhere, vapors being equally dilatable with the gafes, ought alfo to be equally compreffible: but I muft obferve that this latter conclufion cannot be true, unlefs while the compreffed vapors perfectly retain the elaftic ftate; which requires that their temperature be fufficiently elevated to enable them to refift the preffure which tends to make them affume the liquid ftate.

That dry and moift air dilate alike.—Qu.?

I have afferted on the authority of Sauffure, and my own experiments confirm it, that very dry air, and air holding more or lefs of water in folution are equally dilatable; confequently I am authorized from the whole of the facts to draw the following conclufions.

1ft. All

1*ſt*, All the gaſes, whatever may be their denſity or the quantity of water they hold in ſolution, and all vapors, equally dilate by the ſame degrees of heat.

Conclusions. Expanſion of gaſes.

2*d*, The augmentation of volume which each of the permanent gaſes receives, from the temperature of melting ice to that of boiling water, is equal to $\frac{80}{213,33}$ of the primitive volume, if judged by the thermometer divided into 80 parts, and $\frac{100}{266,66}$ of the ſame volume, if by the centigrade thermometer.

Augmentation of volume.

It remains, in order to complete this courſe of experiments, to determine the law of the dilatation of gaſes and vapors, in order to aſcertain the coefficient of dilatation for any degree at a known heat, and to be aſſured of the real progreſs of the thermometer. I ſhall employ myſelf on theſe new reſearches; and when they are terminated, I ſhall have the honour of communicating them to the Inſtitute.

Future experiments on the laws of dilatation at other temperatures.

VII.

New Theory of the Conſtitution of mixed Gaſes elucidated. In a Letter from Mr. J. DALTON.

To Mr. NICHOLSON.

SIR, *Mancheſter, Nov.* 18, 1802.

IN a paper of mine which you had the goodneſs to publiſh in your Journal for 1801 (Quarto Series, V. 241), I announced a new theory of the conſtitution of the atmoſphere. This has ſince been publiſhed on a more enlarged ſcale, and elucidated by a plate, &c. in the Memoirs of the Literary and Philoſophical Society of Mancheſter, Vol. V. Part 2. Notwithſtanding this, I am informed by ſome of my chemical friends, that they do not clearly underſtand the hypotheſis itſelf, and conſequently are not able to judge of its merits or defects: And a late writer (Dr. Thomſon), in his Syſtem of Chemiſtry, Vol. III. Page 270, ſpeaking of the uniform diffuſion of the different gaſes of the atmoſphere, makes the following obſervation: "Even Mr. Dalton's ingenious ſuppoſition, that they neither "attract nor repel each other, would not account for this equal "diſtribution; for, undoubtedly, on that ſuppoſition, they "would

New theory of the conſtitution of the atmoſphere,

not clearly underſtood.

"would arrange themſelves according to their ſpecific gra-"vity." Now as I am perſuaded that no one acquainted with the principles of mechanical philoſophy would have written the above if he had underſtood my hypotheſis, it ſeems to call from me a further explanation. I propoſe, therefore, 1*ſt*, To ſtate in as clear a point of view as the ſubject will admit, the principles which I aſſume: 2*d*, To ſhew that the conſequences which I have deduced from them are legitimate, and particularly that mixed elaſtic fluids ought *not* to arrange themſelves according to their ſpecific gravity: and, 3*d*, To demonſtrate that the ſuppoſition of the gaſes conſtituting the atmoſphere being held in a ſtate of equal diffuſion by chemical affinity, is not only inconſiſtent with the phenomena, but is completely abſurd.

Farther explanation.

Intended order of diſcuſſion.

I. PRINCIPLES ASSUMED.

Principles aſſumed.

1. That particles of ſimple elaſtic fluids repel inverſely as their diſtance.

1. I take for granted that the particles of ſimple (unmixed) elaſtic fluids repel one another with forces inverſely as the diſtance of their centres, the temperature being given. This is a mathematical deduction from the allowed fact, that the ſpace occupied by any gas is inverſely as the compreſſing force: (See Newton's Principia, B. II. Prop. 23). The *abſolute* diſtances of the centres of ſuch particles muſt vary according to circumſtances, and cannot eaſily be determined; their *relative* diſtances in a liquid and aërial ſtate ſometimes may. Mr. Watt has ſhewn, that ſteam of 212° and preſſure 28 inches, is 1800 times lighter than water; conſequently the diſtances of the particles of ſteam are to the diſtances of the ſaid particles in a liquid ſtate, as 12 to 1 nearly, in that particular caſe. Vapor in the vacuum of an air-pump, at common temperature, will have its particles about 4 times the diſtance, or 48 to 1.

2. In mixed elaſtic fluids the heterogeneous particles have no diſtant or gazeſiant repulſion; but only that of contact, formerly called the reſiſtance of impenetrability.

Illuſtration from the viſible effects of magnetiſm.

2. I ſuppoſe that in mixed elaſtic fluids the heterogeneous particles do not repel one another at all at ſuch diſtances as they repel thoſe of their own kind; but that ſuch particles, when brought into actual contact (to uſe the common language), reſiſt each other in all reſpects like inelaſtic bodies. This is the peculiarity of the hypotheſis, and what appears not to be generally underſtood. If I may explain by analogy, the moſt ſtriking will be found in magnetiſm. Two like poles of magnets repel one another with the ſame force whether any other

other bodies intervene or not, and do not affect thoſe other bodies; in the ſame way I conceive two particles of any one gas repel one another with the ſame force whether particles of other gaſes intervene or not, and do not affect thoſe other particles. A magnet is amenable to the common laws of motion in its colliſion with other bodies, and when it is brought into ſeeming contact with them; ſo is a particle of one gas when it is brought into ſeeming contact with a particle of another ſpecies: and in this caſe the bodies may be ſaid to have a repulſive power; but this power is eſſentially different from the others, in that it extends to no definite diſtance. Further, conceive a very fine capillary tube placed perpendicular to the horizon, into which let a number of correſpondent ſmall magnetic wires, or particles, be inſerted with their poles of the ſame denomination together, or more ſtrictly, as near as their repulſive power would admit, one particle above another, the air having intercourſe amongſt them. Then, as the magnetic particles would not actually touch one another, by reaſon of their repulſion, they would *ſeem* to be ſupported by the intervening air, whereas in reality they are ſupported and kept at certain diſtances intirely by the repulſion inherent in themſelves, and their own gravitation: and thus I conceive particles of gas ſupport thoſe of their own kind above them, though, were they viſible, they might ſeem to reſt upon others immediately under them; and the ground, or loweſt ſolid or liquid ſurface, by ſupporting the loweſt particle of each kind, has the weight of the whole to ſuſtain. Theſe obſervations, together with a view of the plate above alluded to, muſt, I think, be ſufficient to ſatisfy any one what the hypotheſis is. And it may be proper to add, there is ſomething much reſembling polarity obſervable in the ultimate particles of bodies at the inſtant of tranſition from the liquid to the ſolid ſtate; witneſs the congelation of water.

Particles of gas ſupport each other by repulſion, in the ſame manner; whether the particles of another gas be preſent or not.

2. CONSEQUENCES.

It is plain from the above account, that I conceive any one gas to be conſtituted of perhaps *one* part ſolid matter, and *one thouſand* or more parts vacuity or pore, if it may be ſo called; and that into this vacuity we may throw as many other gaſes as we pleaſe, without materially diſturbing the firſt, provided we do not abſolutely fill the vacuity with ſolid matter, meaning by

If a gas be one part matter and a thouſand mere ſpace, no difference of effect will follow from other gaſes in that vacuity, &c.

by that expreſſion common liquids or ſolids. Thus we might have had a dozen gaſes in our atmoſphere inſtead of three or four, all in the ſame compaſs, and each retaining the ſame denſity it would have had alone. The heavier gas has no more tendency to raiſe the lighter, than a quantity of ſhot has to expel the air from its interſtices. If therefore Dr. Thomſon, or any other, can ſhew how one fluid, which is not diſplaced nor any way acted on by another, ſhould by its *reaction* cauſe that other to move into a higher or lower ſtation; then the mathematical world will be obliged to reconſider their doctrine of ſtatics. Till then I muſt take the negative of the propoſition, and conclude, that elaſtic fluids of the greateſt and leaſt ſpecific gravity imaginable, on the ſuppoſition I hold, will alike take the loweſt and the higheſt ſtations, regardleſs of each other; or in other words, they will arrange themſelves in the ſame order as if thrown into a complete vacuum. The great difficulty reſpecting the uniform diffuſion of the gaſes being removed, I think on my hypotheſis the other phenomena can require no explanation to any perſon converſant in pneumatics. I will take one inſtance: It may by aſked, How does ſulphuret of potaſh abſtract oxigenous gas out of any mixture; lime water, carbonic acid gas, &c. &c.? The anſwer is obvious: Exactly in the ſame way as if the gas in queſtion was the only one in the veſſel, and the operation going on in a cloſe veſſel.

And one gas will not raiſe any other, whatever may be their individual ſpecific gravities; and the chemical abſorption of any ſingle gas will be the ſame as if it were alone.

3. GASES HELD TOGETHER BY CHEMICAL AFFINITY, ABSURD.

3. It is contrary to fact that gaſes are held together by chem. affinity: For they mix uniformly, and remain ſo; they occupy the ſame ſpace after mixture; and they expand, &c. as before.

On this head it will be proper to premiſe certain facts:

1. When two gaſes of different ſpecific gravity, ſuch as oxigenous and hidrogenous, are put into the ſame veſſel and agitated; if they be ſuffered to ſtand ſome time after, they ſtill continue uniformly mixed.

2. They occupy the ſame ſpace before and after mixture; that is, *one* meaſure of each put together occupy *two* meaſures, the temperature and preſſure remaining the ſame. Mr. Davy ſeems to think this principle is not ſtrictly true in regard to a mixture of azotic and oxigenous gas; but the deviation from it, if any, is extremely ſmall.

3. The compound is ſubject to the ſame laws of rarefaction and condenſation as the ſimples.

There

There are but *three* suppositions we can make essentially different respecting the mutual action of heterogeneous particles of gas: 1*st*, When two gases are mixed their particles may reciprocally *repel* one another, just as they act on their own kind in an unmixed state: 2*d*, They may be neutral, or have neither attraction nor repulsion for each other: 3*d*, They may have a chemical affinity or *attraction* for each other. The advocates for the chemical adhesion of gases will agree with me in exploding the first, because where nothing but *repulsion* is manifest, we can ascribe no effect to attraction: The second, which is the one I adopt, is obviously inconsistent with their hypothesis; and as for the third, I can conceive no other explanation than the following: 1*st*, Two or more heterogeneous particles may unite and become a new centre for the caloric to adhere to; but in this case the gases are no longer *two* but *one*; and oxigenous and hidrogenous would become aqueous vapor: This therefore is not a case of two *gases* being held together by chemical affinity. 2*d*, The two gases may separately retain their caloric and still be held by chemical affinity; that is, there may be an equilibrium between the powers of attraction and repulsion; but this is evidently inconsistent with the third law of condensation and rarefaction observed in such compounds.

Where gases mix, the particles must either, (1.) repel, or, (2.) have no action, or, (3.) attract.

The gases do not *unite* by *repulsion*: if they have no action the author's theory is good; and, if the particles attract and unite they become *one gas*, and not *two*, as in the assumed case:

Or if they attract and do not unite, they must come nearer together, contrary to the third law or fact above stated.

I am, &c.

J. DALTON.

I thank you for the notice you have taken of my Essays, and particularly for the note subjoined to that on the Expansion of Gases. I have lately read a Paper to the Literary and Philosophical Society of this place, in which I have shewn that the quantity of carbonic acid gas found in a given volume of atmospheric air, is not more than $\frac{1}{1000}$ part of the whole; and that the said gas is held in water, not by chemical affinity, but merely by the pressure of the gas abstractedly considered on the surface, forcing it into the pores of the water.

VIII. *A Method*

VIII.

A Method of increasing the Quantity of Light afforded by Candles, and to obviate the Necessity of snuffing them. In a Letter from Mr. EZEKIEL WALKER.

To Mr. NICHOLSON.

SIR,

Feeble light of candles when not frequently snuffed.

WHEN our apartments are illuminated with common candles, which cannot be regularly attended with the snuffers, they produce much smoke, and their glimmering light scarcely serves the most ordinary purposes. My first attempts to improve this *sombre* mode of illumination were made many years ago. Those however were not productive of any useful discovery, but some experiments which I made in the year 1797 afforded results that rather exceeded my expectation.

Method of remedying this in candles for illuminating apartments.

Experience soon convinced me of the utility of the method which I had discovered, of using candles in such situations as above described, but in every attempt to produce the same effect upon my table, I was unsuccessful, till the beginning of the present season. This was owing to an oversight, which might be deemed very unaccountable, did not the history of inventions contain many instances of the same kind.

Great effects are frequently produced by causes that are in general deemed trifles. And, if a trifling alteration be made in the method of using common tallow candles, they will become excellent substitutes for those of wax.

If a candle be inclined it gives a steady light, and requires no snuffing;

A common candle, weighing $\frac{1}{10}$ of a pound, containing 14 single threads of fine cotton, placed so as to form an angle of 30 degrees * with the perpendicular, and lighted, requires no snuffing; and what is much more valuable for some purposes, it gives a light that is nearly uniform in strength without the least smoke. These effects are thus produced:

because the wick protrudes into the air, and becomes burned, &c.

When a candle burns in an inclined position, most part of the flame rises perpendicularly from the upper side of the wick,

* Candlesticks may be made to hold the candle at this angle, or they may be so contrived as to hold the candle at any angle at pleasure. I have candlesticks constructed on both principles.

wick, and when viewed in a certain direction, it appears in the form of an obtuse angled triangle. And as the end of the wick projects beyond the flame at the obtuse angle, it meets with the air, and is completely burnt to ashes; hence it is rendered incapable of acting as a conductor to carry off any part of the combustible matter in the form of smoke. By this spontaneous mode of snuffing, that part of the wick which is acted upon by the flame, continues of the same length, and the flame itself very nearly of the same strength and magnitude *.

The light does not undulate.

The advantages which may be derived from candles that require no snuffing and afford no smoke, may be readily understood; but these candles have another property which ought not to be passed over in silence. A candle snuffed by an instrument gives a very fluctuating light, which, in viewing near objects, is highly injurious to the eye; and this is an inconvenience which no shade can remove. But when a candle is snuffed spontaneously, it gives a light so perfectly steady and so uniformly bright, that the adjustments of the eye remain at rest, and distinct vision is performed without pain, and without uneasiness.

Candles which were subjected to experiment.

Candles on which I have made experiments, are described in the following

TABLE.

No.	No. of candles to the pound avoirdupois weight.	Length in inches.	No. of single threads of fine cotton in the wick.
1	14	8.5	10
2	12	9.	12
3	10	9.75	14
4	8	10.	20
5	6	10.25	24
Mould	6	13.	

Numbers 1, 2, and 3. These candles, when lighted and placed to form an angle of 30° with the perpendicular, require

* The wick's not being uniformly twisted throughout, may occasion a little variation in the dimensions of the flame.

 no

no ſnuffing: they give lights which are nearly equal, and combuſtion proceeds ſo regularly, that no part of the melted tallow eſcapes unconſumed, except from accidental cauſes.

Remarks on the phenomena.

No. 4, placed at the angle mentioned above, and lighted, requires no ſnuffing: it gives a light very little ſtronger than No. 1, but its colour is not quite ſo white, nor its flame ſo ſteady.

No. 5. This candle, placed at an angle of 30°, and lighted, requires no ſnuffing; its flame is rather fluctuating, and not ſo white as No. 4, nor is its ſtrength of light much greater than No. 1. The melted tallow ſometimes overflows when the air in the room is put in motion; yet the light of this candle is much improved by the candle's being placed in an inclined poſition.

The mould candle, treated in the ſame manner as thoſe above, affords a very pure ſteady flame, without ſmoke and without ſnuffing, and its ſtrength of light is about equal to that of No. 1.

ON ECONOMY.

The economy of candles as to light and conſumption of the fuel.

My experiments have not been ſufficiently numerous to determine with preciſion which of theſe candles affords the moſt light at a given expence, but the few experiments which I have made ſeem to indicate, that the quantity of light is directly as the quantity of combuſtible matter expended.

EZ. WALKER.

Lynn, Nov. 19, 1802.

REMARKS.—W. N.

On candles in general.

On candles in general ſee an eſſay by W. N. in the Philoſ. Journal, quarto, I. 67; alſo my Firſt Principles of Chemiſtry, Art. *Wax:* Hermſtadt on the different materials, wax, tallow, ſpermaceti; Philoſ. Journ. quarto, V. 187.—I have always ſuſpected from the general facts, that the light of ſmall candles is cheaper than that of larger, and have little doubt of the truth with regard to ſuch as exceed the diameter of three quarters of an inch in tallow. We are much in want of a ſeries of ex-

Outline of very deſirable experiments concerning them.

periments on the different deſcriptions of candles, under the general

general titles of Wax, Spermaceti, Tallow, and mixed; and tabulated, to ſhew, 1. Average weight of the wick; 2. Number of threads; 3. Weight of the whole candle; 4. Diameter; 5. Length; 6. Time of combuſtion per inch; 7. Ditto per ounce; 8. Quantity or intenſity of light when newly ſnuffed; 9. Ditto after burning half a minute, or till the uſual period of ſnuffing; 10. Average intenſity; 11. Expence of material per hour to produce the quantity 1.00 of light; 12. Ditto in the money price of the article; 13. Station of the barometer; 14. Ditto of the thermometer; 15. And ditto of the eudiometer. I am abſolutely unacquainted with any good ſtandard of compariſon of light. The ſun perpetually varies with the weather, its altitude, &c. It ſeems probable that an earthen lamp, having a wick of known texture and weight, cut to a determinate elevation by a gage, and charged with very pure olive oil, might, at like ſtations of the barometer and thermometer, give light ſo nearly equal, for the firſt two or three hours, as to afford reſults conſiderably exact; of which the means of ſeveral repetitions would be of great practical utility. The compariſon of lights by the ſhadows has been often mentioned; but the ſimplicity and value of the method (not yet univerſally known) will juſtify a ſhort repetition. When the ſhadows of the ſame object fall upon each other, not preciſely, the bordering ſhades will be ſeverally illuminated by one light only: move the lights till theſe bodies appear equally dark at the point or place where they meet in an angle. Then meaſure the diſtances of the lights from that point or place, and the intenſities are inverſely as the ſquares of thoſe diſtances. It is to be noticed that the obſtacle which forms the ſhadow, a book for example, muſt be held ſo that the ray from each light may fall on the wall nearly in the ſame angle; and this will always be the caſe if the farther light be nearly behind the other.

Standard of the intenſity of light unknown.

Method of comparing lights by the ſhadows.

In a ſubſequent letter, Mr. Walker informs me, that he carefully meaſured his intenſities by the method of ſhadows, as mentioned in Prieſtley's Optics. I conclude this to have been the method here deſcribed; though, for want of a good index, I can only find the method of Bouguer in that work.

IX.

Description of a new Secret Lock. By J. B. BERARD.

(Concluded from Page 222.)

Conftruction of a fecret lock acceffible only to the proprietor.

I MAY alfo in this place ftate the reafon why I have made the key as well as the ferrils with 24 rather than 25 divifions. In the firft place, it affords a fmall diminution in the dimenfions of the lock. Another more effential advantage is, that in the divifion of 24, each notch in the ferril is oppofed to another diametrically oppofite, which facilitates the introduction and taking out of the bolt. We may obferve, that for the fame reafon the teeth of the ferrils, being rather rounded at the tip, facilitate the entry of the little wedge, which is likewife rounded underneath.

The lock being thus conftructed, it only remains to fix it to the door. For this purpofe, an opening is contrived of the fize of the box of the lock, in the horizontal crofs piece, without cutting the vertical frame of the door; and round it a groove is made, of 2 in depth and 6 in breadth, on which the face of the lock refts: the lock is placed in this opening without the bolt; and if it is defired, four nails or fcrews may be driven into the wood through four holes made in the face of the plate; but this is not neceffary. The bolt is then placed on the other fide of the door, and if the wood be too thick, it may be cut away till the bolt flides fair and eafily over the wood. This is very eafy to be done, and then the face of the bolt ought to lie flufh with the edge of the door.

It is obvious that the height of the ferrils muft be proportioned to the thicknefs of the door. If the door be 30 m. thick, for example, the ferrils (which ought to be 1 under the furface of the lock) would be about 29.

I muft alfo obferve, that in order to render the motion of the fcrew-heads more eafy and uniform, a pafteboard of 1 in thicknefs, with five holes punched in it, muft be laid upon the inner furface of the plate of the lock; and above the pafteboard, another flip of very thin brafs, likewife perforated with five holes: then the interior flat wheels have a very eafy motion

over

over the brafs which refts on an elaftic plane, which yields to the unavoidable irregularities of execution in a work of a moderate price.

Conftruction of a fecret lock acceffible only to the proprietor.

It muft likewife be obferved, that the heads of the fcrews, as well as the face itfelf of the lock, ought to be cafe hardened, in order to protect it from external injuries, and to prevent the enlargement of the holes which receive the fcrews. The furface ought to be highly polifhed, and the heads of the fcrews fhould be blue.

Advantages of the new Lock.

1*ft*, As in Reignier's lock, which likewife has fcrew-heads, ferrils, and teeth without wedges, thefe pieces cannot be put at diftances extremely precife, it neceffarily follows that one of the pins or teeth, the fourth for example, will always meet its ferril before the others meet theirs. Let us fuppofe that with a turnfcrew we endeavour to turn the fcrew-head which moves the bolt, whilft with another hand and turnfcrew we fucceffively turn the other fcrew-heads, they will all turn freely, except a fourth, which will be immoveable: It will be concluded that the fourth fcrew-head is not in the pofition to open the door, that is to fay, that the deep cut of the ferril is not oppofite the pin of the bolt: that figure or letter of the fcrew-head will be noticed among thofe which do not form the fecret. The pofition of the fcrew-head muft therefore be changed, and the above procefs repeated till the pin is felt to fall into the notch. The fame experiment muft then be made with that fcrew-head among the remaining ferrils which alone is now preffed by its pin. And after eighty attempts at moft, if there be eight fcrew-heads, each having eleven figures, we fhall know the eight figures which compofe the fecret.

Here then is a regular and infallible procefs to difcover in lefs than an hour the combinations with which the lock is clofed. This defect, which is common to the generality of compounded locks, does not exift in mine.

When my bolt is moved, the wedges which precede the teeth enter at the fame time into the cuts of the ferrils, which are of fuch a height that they graze the interior furface of the lock: all the four ferrils then become immoveable, and it is impoffible to difcover which is the ferril that is firft met by the correfpondent tooth, becaufe their fixation is produced at the fame

Construction of a secret lock accessible only to the proprietor.

same time from the teeth and the wedges. No external examination can therefore shew the difference in situation of the four ferrils. It is obvious that in order to insure this effect, the wedges ought to fit the cuts between the teeth in the ferrils exactly; which is very easily obtained in the construction. Another advantage resulting from the wedge is, that if any of the deep notches or openings be not exactly in a line with the rest, the wedge easily adjusts it, and the bolt is not in the least impeded on that account; this advantage renders it unnecessary that the workmanship of the lock should be exquisite, a condition which, in a work of a moderate price like the present, would render it useless.

2d, My lock is easily opened, it having only four screw-heads: it has nevertheless 331,776 * combinations, a number fully sufficient to set the most suspicious miser at ease.

We may add, that it is much easier to recollect one word than a number, which must be the case in Reignier's lock.

3d, When characters are engraven externally on the screw-heads, they are either difficult to be perceived, or may be easily damaged. A key with characters engraved on it, is I think a great improvement: a saving is likewise afforded, which is a consideration of some consequence in a machine of common use.

4th, The method by which I vary the position of each ferril with respect to its screw-head, is at once simple and solid.

5th, The mechanism by which the bolt is attached to the lock, is concealed, and cannot be damaged. As nothing is seen of the lock but a smooth plate on either side, it would be very proper for a bureau or 'scrutoire.

Another singular property resulting from this mechanism is, that it can only be taken to pieces by the proprietor, whether the door be open or shut: it could not even be done by a person shut in the apartment.

6th, The lock may easily be set: as the interior screw-heads do not turn on wood, there is no danger that their motion should become less regular or easy on account of wear. The same may be observed of the bolt, which slides in a groove of iron.

* This is the fourth power of 24.

7th,

7th, The lock is very ſolid; for there is no opening into which any tool might be introduced. The face of the door may likewiſe be ſurrounded with a ſtrong metallic covering let into the wood, and the back part of the door may be covered with iron plates, the only method of preventing any one from cutting the wood round the lock.

Conſtruction of a ſecret lock acceſſible only to the proprietor.

8th, If it ſhould be thought inconvenient to place the lock in the thickneſs of the door it may be eaſily avoided. The ſtems of the ſcrew-heads need only be made longer, and another plate ſimilar to that of the face, added: the ſcrews in that caſe, after having paſſed through the face as well as the door, would paſs into the lock. This addition requires an increaſe of work, but it affords no real advantage.

Lock for a Bureau or Secretary.

Though my lock, as I have deſcribed it, can be eaſily fixed to a ſecretary, yet I think it uſeful to indicate the changes convenient to be made in it.

1ſt, The height of the ferrils muſt be diminiſhed, that the bolt may be intirely ſunk in the flap of the ſecretary.

2d, In order to diminiſh the length and breadth of the lock, ten diviſions in the ferrils may be made inſtead of eighty: they need then be only 25 in diameter; but then the ſecret will conſiſt in a number of four figures inſtead of a word of four letters, which is not ſo convenient, as I have already obſerved.

3d, The box of the lock may be ſuppreſſed, and the lock will ſtill have ſolidity ſufficient for a bureau; the price will likewiſe be diminiſhed.

4th, Inſtead of engraving characters on the face of the lock, I prefer, as has been ſeen, to mark them on the turnſcrew itſelf; but the external damage is not to be feared in a ſecretary as in a door. It is therefore more convenient that the figures or letters ſhould be on the faee itſelf of the ſcrew, and a knob or ſmall handle may be contrived on the face of the ſcrews, in order to turn them. In this caſe no key or turnſcrew need be carried about; this is an advantage of ſome conſequence.

Concerning ſome other Kinds of Compound Locks.

It is poſſible, as I have already obſerved, to apply the principles of theſe combinations to the conſtruction of locks in a number

Conftruction of a fecret lock acceffible only to the proprietor.

number of methods. That which I have defcribed is the laft refult of my endeavours on this object; it feems to me to be the fyftem that beft anfwers all the conditions of the problem: I think, however, that it will not be ufelefs to make known fome more, if it were only to prevent others from making unneceffary attempts on the fame fubject.

1ft, The front part of the lock prefents 25 fmall rectangles, 10 wide by 15, difpofed in a right line, and forming in all 250. The 24 letters of the alphabet are marked on thefe rectangles: they are included between the two partitions of the horizontal channel or groove, and they all touch one another. They may be pulled outwards to the diftance of 4 millemetres, like the ftops of an organ. Each of thefe ftops has a little fpring at top, which gives it an eafy and fmooth motion, and prevents it from dragging with it any of its neighbours. Each ftop, in the infide of the lock, has a projection in the form of a tooth.

The bolt has, on its fuperior and horizontal face, 24 dovetailed grooves with dovetails, each of which receives a regifter or ftop of the fame form, having a tooth at top. Thefe ftops of the bolt may be put into their grooves with the extremity having the tooth, or the other end, juft at pleafure. The bolt is enclofed in a box, the vifible and interior face of which moves in a groove, in order to take out and replace the bolt at pleafure.

The lock is thus ufed. If it is to be opened, the exterior ftops forming the fecret are brought forward, then the 25th ftop is drawn, and this renders the reft of the ftops immoveable. After having returned it, it is turned by means of a knob or handle which it carries, and the bolt is thus made to move.

When the fecret is to be changed, the interior regifters are to be placed in the grooves cut in the bolt, by the extremity which has no tooth. They are then inverted with refpect to the others, and their teeth become in a line with thofe of the exterior ftops.

The number of combinations in this lock is equal to the eightieth power of 2, nearly 17 million: It is convenient for a bureau, but the workmanfhip muft be exquifite: Befides, it is not convenient for a door, becaufe ftops which project feveral millimetres may be eafily damaged. I cannot here enlarge upon the other imperfections which it prefents. In this fyftem the

the 24 ſmall ſcrew-heads might be ſubſtituted for the ſtops, which would not then require ferrils. This arrangement would not be preferable to the other.

Conſtruction of a ſecret lock acceſſible only to the proprietor.

2*d*, Let us imagine three rings lodged one within the other, and forming one and the ſame plane with a fourth fixed to the door, and an immoveable circle in the centre; ſuch is the face of the lock. The three intermediate rings turn freely and ſeparately by means of a button, which muſt be made to correſpond with one of the letters of the alphabet marked on the outer circle fixed to the door. Each carries interiorly a ferril capable of 24 poſitions. When the notches of the three ferrils are diſpoſed in ſtraight lines, the bolt is made to move by means of a knob or handle in the middle of the face.

This lock* reſembles in ſome reſpects the firſt I have deſcribed. In this the ferrils are concentric, and in the others they are ſeparate. It has an agreeable form, and would be proper for a ſecretary, and its execution does not require much preciſion. It is not very convenient for a door, becauſe it is likely to be damaged.

3*d*, Let us imagine five ſcrew-heads on the face; let each of theſe ſcrews carry a ſmall pinion within, which moves a notched bar or rack. This rack muſt have ten teeth, one of which is withdrawn, in order to leave a clear way or ſpace. The fifth ſcrew muſt move a fifth rule carrying four teeth, and at right angles to the other four. It is clear that this laſt rule, which I ſuppoſe to be the bolt, can never move except when the other four rules are in a convenient poſition. In this method the lock will be enormouſly large; and it will alſo be very difficult to prevent this ſecret from being diſcovered from without.

4*th*, If toothed wheels, with an unequal number of teeth all taking in one another, be ſubſtituted for the flat wheels which are under the ferrils in the ſyſtem of locks with ſcrew-heads, then, if one ſcrew-head be moved, it will derange all the ferrils at once. This ſaves time, without diminiſhing the number of combinations. This advantage led me to approve of the conſtruction for ſome time; but it is productive of ſo many ſerious inconveniences, that I was obliged to abandon it.

* I have ſeen ſome at Paris, nearly ſimilar to this.

It

Conftruction of a fecret lock acceffible only to the proprietor.

It would be fuperfluous to examine a greater number of fyftems. I fhall conclude by one general obfervation.

All the compound locks that can poffibly be made, may be divided into three fpecies. In the firft, ftraight or curve lines are made to move; in the fecond, plain or curved furfaces; in the third, folids. It is obvious that one folid offers as many arrangements or combinations as three lines together, and one furface as many as two lines.

Thefe lines, furfaces, and folids, form interior ftops made to receive motions (externally produced) in a ftraight or in a curve line.

It is obvious from the application of thefe principles, an almoft infinite variety of locks may be conftructed. I fhall not undertake to extend the variations, but fhall only fay, that I think I am convinced that the beft fyftem is that of moving the circumferences of circles internally by circular motions produced from without.

CONCLUSION.

I am fenfible that what I have faid concerning the locks, which I confider as lefs perfect, is not fufficient to direct their conftruction; but I have faid enough to make their fyftem known to artifts, and to deter them from fruitlefs experiments. It muft not be forgotten, that in this kind of machine nothing good or ufeful fhall be done, unlefs fimplicity, convenience, folidity, economy, and facility of execution, be combined.

It is under thefe feveral points of view that the problem of compound locks, which may be worthy of the attention of artifts, ought to be confidered. I do not pretend to have difcovered the beft, but I dare affert, that the lock which I have defcribed approaches fo near to perfection, as to entitle it to the fuperiority over the common locks which are more or lefs ingenious and complicated, and even to place it among the locks of combination, as one of the moft fimple that has yet appeared, or that I know of.

Whether I fhall be allowed the fmall honour of having invented this arrangement, or the merit only of having brought it to perfection, is not to me a matter of confequence. I fhall be fatisfied if I have affifted others in improving this fpecies of machines, the utility of which need not be proved. In fact, the common locks which are brought to the greateft perfec-

tion,

tion, were rendered as ſure and inviolable, as they are at preſent the contrary, they would ſtill have this inconvenience: they are liable to be forced by introducing ſome tool or inſtrument; and even if the violator ſhould fail in his attempt, the proprietor will nevertheleſs be reduced to the neceſſity of breaking open his own door if the inſide of the lock ſhould be ever ſo little deranged. The advantage then of a lock which affords no entrance to a key is obvious, a problem which I think cannot be well ſolved, except by a circular motion. That which I have deſcribed, not only anſwers all the conditions of the theory, but has ſtood the teſt of experience. The greateſt obſtacle to the propagation of locks of combination, proceeds from the prejudices of habit, ſtill more than from their want of ſimplicity: and it is well known that this is the fate of all truths *.

X.

Deſcription of Dr. YOUNG'S *Apparatus for exhibiting the Colours of thin Plates, by means of the Solar Microſcope. From the Journals of the Royal Inſtitution,* I. 241.

The colours of thin plates have been little examined, and never publicly exhibited.

THE colours of thin plates were obſerved by Boyle and Hooke, and more accurately analyſed by Newton: but little or nothing was added to the account that Newton gave of them, until ſome attempts were lately made to explain them, and to build at the ſame time on the explanation, the principal arguments in favour of a new ſyſtem of light and colours. The phenomena themſelves were very little known, except from Newton's deſcription; it had happened but to few to obſerve them: and they had never been made conſpicuous to a public audience in a form equally beautiful and intereſting.

New attempt to exhibit them by the ſolar microſcope.

It appeared, however, that there would be little difficulty in applying the apparatus for repreſenting opaque objects in the ſolar microſcope, to the exhibition of theſe colours on a large ſcale: but ſeveral precautions were neceſſary, in order to obtain the moſt advantageous repreſentation; and, theſe precautions having been completely ſucceſsful, it may be of ſome utility to give a detached account of them.

* In the former Paper for Requier read Reignier, and in the note on p. 217, for Braviali read Bramah.

The

Methods of producing the colours.

The colours of thin ſubſtances muſt often have been ſeen in bubbles of water or of other fluids, and in the film produced by a drop of oil ſpreading on water; they were more particularly obſerved in the plates of talc, or of ſelenite, into which thoſe ſubſtances readily divide. Sir Iſaac Newton made his experiments principally on the colours of ſoap bubbles, and on thoſe which are produced by the contact of two lenſes. For inſpecting the colours of ſoapy water, the moſt convenient method is that of Mr. Jordan. He dips a wine glaſs into a weak ſolution of ſoap, and then holds it in a horizontal poſition againſt an upright ſubſtance, for example, a window ſhutter; the film covering the glaſs being in a vertical poſition, the gravity of the fluid tends to make it thicker at the lower part, and it becomes every where gradually thinner and thinner, till at length it burſts at the uppermoſt point. The colours aſſume, in this caſe, the form of horizontal ſtripes, ſimilar to the rings which are to be more particularly deſcribed.

Mr. Jordan's convenient method of exhibiting the ſoapy film.

The colours from a denſe plate are more vivid than thoſe from a rare one; becauſe the latter are diluted by the light reflected from the confining mediums.

It has been obſerved by Newton, that the colours thus reflected from a plate of a denſer medium, are more vivid than when a plate of a rarer medium is interpoſed between two denſer mediums. But the cauſe of this apparent difference is probably, the quantity of foreign light that is generally preſent in the experiment, reflected as well from the upper ſurface of the ſuperior medium as from the lower ſurface of the inferior, both theſe ſurfaces being often nearly parallel to the ſurfaces in contact. It becomes therefore deſirable to remove this foreign light: this may be done effectually, by employing one glaſs in the form of a priſm, and coating the lower ſurface of the other with black ſealing wax: the light reflected by the oblique ſurface of the firſt, is thus thrown into another direction; and the reflection of the inferior ſurface of the ſecond, is either deſtroyed or rendered imperceptible. And, with theſe precautions, the rings of colours produced in the reflected light, may be rendered a very beautiful object by means of the ſolar microſcope.

Remedy.

The plane glaſs is made priſmatic, and the lower ſurface of the lens coated black.

The moſt perfectly plane glaſſes are thoſe which are uſed for Hadley's quadrants: one of theſe may be ground in the direction of the diagonal of its tranſverſe ſection, ſo as to make a thin wedge or priſm; and the ſurface of the lens employed muſt be a portion of a ſphere of from five to ten feet radius.

The

The two glaſſes muſt be retained in their poſition by means of three ſcrews; for, as ſoon as the preſſure is removed, they repel each other with conſiderable force; and, for this reaſon, neither of them ought to be very thin, otherwiſe they will bend before they are ſufficiently near.

For adjuſting the glaſſes to the microſcope, it is convenient to fix them in a cylinder of ſufficient ſize to project beyond the glaſſes and their ſcrews, in order that they may be readily turned ſo as to reflect the light coming from the ſpeculum into the direction of the axis of the microſcope: it is obvious that, in this caſe, they muſt be ſomewhat inclined to the light, ſo that the focus of the whole image will never be equally perfect; and, inſtead of being circular, like the rings themſelves, their images on the ſcreen will be oval. In this manner, eight or ten alternations of colours may eaſily be obſerved; but their order and ſequence is too complicated to be eaſily underſtood: for they are really compoſed of an infinite number of ſeries of rings of different magnitude, each ſeries being formed by each of the gradations of light in the priſmatic ſpectrum, which, near the centre, are ſufficiently ſeparate to form diſtinct appearances, either alone or in combination; but, after eight or ten alternations, are loſt in the common effect of white light. For, when the glaſſes are illuminated by homogeneous light only, ſeparated from the reſt by the refraction of a priſm, or otherwiſe, the rings of each colour occupy, together with the dark ſpaces, the whole viſible ſurface, their number being only limited by the power of the eye in perceiving objects ſo minute as the external ones become, in conſequence of the rapid increaſe of the thickneſs of the plate of air near the edges of the curved ſurface. This circumſtance being once underſtood, it is alſo capable of being illuſtrated in a manner ſtill more elegant, by placing a priſm a few feet from the microſcope, leaving only a narrow line of its ſurface expoſed to the incident rays, and then throwing the rings of colours on it, in ſuch a direction that this line ſhall paſs through their centre. Care being taken to exclude from the priſmatic ſpectrum thus formed all extraneous light, it exhibits a moſt intereſting analyſis of theſe colours; for the line conſiſts of portions of the rings of all poſſible gradations of colour, each forming a broken line, but not of the ſame dimenſions; and, by the priſmatic refraction, all theſe broken lines are ſeparated and placed parallel

This apparatus may be fixed in the ſolar microſcope.

The effect

with homogeneous light,

and ſtill more elegantly by a priſm.

Intereſting effect.

rallel to each other, on account of the different refrangibility of the light of which they confist. Thus the broken line of the extreme red, which confifts of the longeft portions, is leaft refracted: the other reds follow, and are placed in contact with the firft, and with each other, but on account of the different magnitude of the portions, fomewhat obliquely. The dark fpaces alfo are in contact, and form a feparation between each portion of light. In the fame manner, the green follows the red, with little or no vifible yellow. The blue and violet are fomewhat mixed: for thefe two colours are much lefs widely feparated by thin plates than by the prifm: for this reafon, each portion of light formed by the contiguous lines of the different colours is bounded not by ftraight but by curved lines.

It is evident that, by drawing a line acrofs this compound fpectrum at any part, we may learn the component parts of the light conftituting the rings at that part; for the prifm only fpreads the colours in a direction tranfverfe to this fpectrum: and it may be obferved, that after the eighth or tenth alternation, the light tranfmitted at each point is fo mixed, that we may eafily underftand how it appears white.

Colours by tranfmiffion are not fo advantageoufly fhewn.

The colours of thin plates, as feen by tranfmiffion, are alfo eafily exhibited in the folar microfcope; but, fince it is utterly impoffible to exclude the very great proportion of the light which is not concerned in their formation, they never appear fo brilliant as the colours feen by reflection.

Y.

XI.

Extract of a Letter from R. CHENEVIX, *Efq. on the Magnetic Property of Nickel.*

Nickel is magnetic.

I AM afraid that I was too hafty in denying the magnetic property of nickel. The fact is, I had obtained both it and cobalt in that ftate. But in profecuting a feries of experiments upon thefe metals, I perceived the real caufe why feveral fragments which I had obtained did not exert any influence upon the magnet. I took a piece of nickel which was not attractable, and which exhaled arfenical vapours when

heated

heated by the blow pipe, and diffolved it in nitric acid. I boiled the folution until the whole of the metal was converted into arfeniate of nickel; I then dropped in a fufficient quantity of nitrate of lead, and evaporated the liquor at a very gentle heat, but not quite to drynefs. Alcohol poured into this folution precipitated every falt but the nitrate of nickel, which had been formed by the double decompofition of the arfeniate of nickel and the nitrate of lead. This is the method which I ufed in the analyfis of the arfeniated copper ores of Cornwall; and the proportion of arfenic acid, contained in arfeniate of lead, was the ftandard to which I referred thofe ores to afcertain their relative proportions. Thefe experiments were publifhed in the Philofophical Tranfactions for 1801.

Examination of a piece not magnetical.—Solution in nitric acid; ebullition; nitrate of lead added; evapor. nearly to drynefs; alcohol added which diffolved nitrate of nickel only;

After the preceding operations, I evaporated the alcohol of the folution of nitrate of nickel, rediffolved the metallic falt in water, and decompofed it by potafh. The oxide, well wafhed and dried, was reduced in a Heffian crucible, lined with lamp black; and in that ftate it manifefted ftrong magnetic properties. None of the proceffes of the reagents I had ufed could have communicated iron to the nickel. I then allayed this fame nickel with a fmall proportion of arfenic in a fimilar crucible, and found that it had loft its magnetic property. A fmall portion of iron added to the mafs did not render it attractable. From thefe experiments, often repeated, I believe that arfenic has the power of concealing the magnetic property of nickel. Cobalt appears to be in the fame cafe. I was therefore miftaken when I announced, that thofe metals were not in themfelves magnetic.

evap. of the alcohol; folution in water; precip. by potafh; reduction *with charcoal? It was ftrongly magnetic.* Arfenic being added deftroyed the magnetic property. A little iron did not revive it.

XII. *On*

XII.

On the unexpected Production of Sulphate of Magnesia. By J. BOSTOCK, *M. D.*

To Mr. NICHOLSON.

SIR,

Liverpool, Nov. 18, 1802.

I FEEL somewhat doubtful whether the subject of the following communication may appear to you sufficiently interesting, or the experiments detailed with a sufficient degree of accuracy, to entitle it to a place in your Journal. I have however sent it for your inspection; if you think it of any value, it is completely at your service.

I remain,

your's, &c.

JOHN BOSTOCK.

Copious saline efflorescence on the inner surface of a brick wall.

ABOUT two years ago, a house was erected in the neighbourhood of this town by Tho. Earle, Esq. upon his estate of Brook Farm. The walls, which were chiefly constructed of stone, were lined with bricks, formed of clay procured from the foundation. Some months after the erection of the building, a large part of the bricks were covered with an extremely copious, saline efflorescence. Mr. Earle felt anxious lest this peculiar substance should prove injurious to the plaister, with which it was proposed to cover the walls, and was desirous to be informed respecting its nature. With this intention the following experiments were performed.

Analytic experiments to determine its nature.

The salt was dissolved in water, filtered, and gently evaporated; it crystallized with difficulty, and the crystals were too indeterminate to ascertain precisely their figure. The salt thus purified had a bitter, acrid taste; it neither effloresced nor deliquesced after being exposed for several months to the atmosphere. It was very soluble in water; 100 grains of water at the temperature of 55°, dissolved nearly 50 grains of the salt; and the same weight of water at the boiling heat, dissolved nearly three times that quantity.

1. To

1. To a dram of the aqueous ſolution, 20 quarts of ſolution of the muriate of barites were added, and a copious, denſe, white precipitate was inſtantly produced.

2. Equal parts of the aqueous ſolution and lime water, when added together, formed a cloud which gradually ſubſided.

3. Twenty drops of pure potaſh were added to a dram of the ſolution, and a copious precipitate was inſtantly produced.

4. To a dram of the ſolution were added two drams of a ſolution of the carbonate of potaſh, a flocculent precipitate was immediately produced.

5. A very copious precipitate was produced, when twenty drops of pure ammonia were added to a dram of the ſolution.

6. The oxalic acid produced no action when added to the ſolution.

7. Ten drops of the nitrate of ſilver being added to the aqueous ſolution, produced ſcarcely any effect; a ſlight milkineſs only was perceptible in the fluid.

From theſe experiments the nature of the ſalt is ſufficiently evident; the action of the muriate of barites proves it to be a ſulphate, and the experiments with lime water and the alcalis, ſhew that the ſulphuric acid is united to magneſia. From the 6th experiment we find that it contains no lime, and from the 7th, that it contains only a very minute portion of muriatic acid. It appears therefore to be the ſulphate of magneſia, or Epſom ſalt, in almoſt a perfectly pure ſtate. The ſulphate of magneſia, which is uſually met with in commerce, contains a conſiderable proportion of muriatic acid; this is proved by the very copious precipitate which it will be found to yield upon the addition of the nitrate of ſilver. To the muriatic acid which it contains, Bergmann aſcribes the tendency which the common ſulphate of magneſia poſſeſſes, of attracting moiſture from the atmoſphere.

It was ſulphate of magneſia.

Remarks.

The reſult of theſe experiments were not what I expected; I imagined that the efflореſcences which we occaſionally obſerved on walls, were compoſed for the moſt part of the nitrate of potaſh. The purity of the ſalt was alſo a circumſtance which appeared curious, as being contrary to the ſtate in which we uſually find it. The moſt ſingular circumſtance, however, was its appearing at all in that ſituation.

The ſalt was not found in the clay of the bricks.

Upon an examination both of the water and the clay, with which the bricks were made, there are but faint indications of the exiſtence of this ſalt, at the ſame time that the preſence of the muriatic acid is very perceptible. Is it poſſible that the muriatic acid may have been diſſipated during the burning of the bricks, and the ſulphuric generated by the union of oxigen with ſome ſulphureous matter exiſting in the coals? I fear this conjecture will be thought extravagant, but at preſent I ſee none which is more probable.

Conjecture.

This incident did not injure the plaiſter.

It is neceſſary to add, that no injury to the plaiſter appears to have ariſen from the preſence of this ſulphate of magneſia.

XIII.

Abſtract of a Memoir on the Dew. By CIT. BENEDICT PREVOST *.

Well known fact that dew falls more plentifully, or in preference, on glaſs and not on metals.

IT is known that a glaſs veſſel is ſometimes covered with dew in the midſt of a veſſel of ſilver, which at the ſame time remains dry; and that mercury in a china veſſel is not lowered by the dew, though the edges of the veſſel are very wet; and it is very generally concluded from various experiments of this kind, that glaſs is a ſubſtance upon which the dew is moſt plentifully depoſited, while it is not at all precipitated on metals.

Repetition of the experiments.

Cit. Prevoſt was curious to repeat theſe experiments, which did not ſeem to him to have been made with ſufficient care, in order to determine to what degree of preciſion it might be allowable to ſupport the uſual concluſion. In this repetition he obſerves ſome very ſingular facts, of which we ſhall proceed to give the general reſults.

Diſcs of metal upon glaſs were often changed with dew; tho' more frequently they are dry and the glaſs wet.

1. Diſcs of tinfoil, of gold, of ſilver, of copper, &c. being applied or ſtuck to plates of glaſs, and expoſed to the dew, were often found to be no leſs charged with dew than the glaſs itſelf on which they were placed; though it more commonly happens that they remained dry, while the glaſs was very wet.

* Read to the Society of Sciences and Arts of the Department of Lot, ſitting at Montanban. Tranſlated from the Annales de Chimie, XLIV. 75.

2. In

2. In this caſe the preſerving property of the metal frequently extends to a conſiderable diſtance round, ſo that in the midſt of the humidity, there is conſtituted a zone perfectly dry and almoſt always well terminated. But what is very remarkable is, that when the glaſs is moiſtened underneath, the place anſwering to the metal remains dry; ſo that the influence of the metal acts through the glaſs of ſeveral millimetres (or twentieths of an inch) in thickneſs. And we ſhall ſoon ſee that it extends much farther.

The metal is not only dry, but cauſes a dry place round it, and even on the oppoſite ſurface of the glaſs.

3. The metal does not in this caſe act as a ſhelter or defence, for if the ſurface be covered with glaſs nothing particular happens.

An additional glaſs plate does not produce the effect of the metal.

4. Having paſted (*collé*) againſt the upper part of a pane of glaſs in a window expoſed to the north, and on the inner ſurface a metallic rectangle; and on the lower part of the ſame pane externally a ſimilar rectangle, the author obſerved, that when the moiſture was condenſed on the inner ſurface, as often happens in winter, the inner metal ſo far from remaining dry, was more moiſtened than the glaſs, while the ſurface correſponding with the external metal was perfectly dry, as well as a ſquare of ſeveral lines round about. It was however remarked, that the humidity in the ſquare approached much nearer the corners of the metal than in the middle of the ſides of the rectangle.

A metallic plate being paſted on the pane of a window within; and another on a different part without; moiſture condenſed within fell moſt on the inner metal, and none on the ſurface correſponding to the outer;

3. In the ſame arrangement if the humidity was depoſited on the outſide (by cooling) the external metal remained dry, and the place correſponding with the internal metal was on the contrary rather wetter than the reſt of the glaſs.

and moiſture without fell only on the ſurface oppoſite the internal metal, and none on the outer metal;

4. When the humidity was formed both within and without, no moiſture was depoſited either upon the external metal, or its correſpondent place within.

moiſture on both ſides did not wet the external metal; or its place within.

7. When a ſmaller rectangle was paſted on the outer ſurface in the middle of the place correſponding with the interior rectangle, and humidity was depoſited internally, the interior rectangle was not entirely wetted as it would elſe have been, but the place correſponding with the ſmall external rectangle remained dry.

A ſmall external metal on the middle of the ſurface oppoſite the internal metals, produced dryneſs on the oppoſ. metal.

8. If upon the ſmall external rectangle a diſc of glaſs be applied, a round wet ſpot is formed within in the middle of the dry place of which we have juſt ſpoken, correſponding to the place occupied externally by the glaſs.

A ſmall external glaſs plate in the middle of the external metal, produced wetneſs within.

9. If

If the whole external metal be covered with glafs, its effect ceafes.

9. If inftead of this laft mentioned fmall difc, a glafs of the fame fize as the external metallic difc be applied fo as exactly to cover this laft, the humidity is difcovered over the whole of the interior rectangle in the fame manner as if the glafs were bare on the outfide.

Metal upon this laft glafs plate revives the effect.

a. If upon the rectangle of glafs there be applied another metallic rectangle, no humidity is again produced on the corresponding interior furface, though it continues to be formed on the fame fide on the reft of the metal and glafs.

Another glafs deftroys it.

b. But the humidity is again condenfed, if on the laft external metallic rectangle there be applied a rectangle of glafs.

Another metal revives it, &c.

c. And this again difappears, or ceafes to be produced, by the application of a third metallic leaf.

d. A third rectangle of glafs caufes it again to appear.

e, f, g, &c. &c. &c.

This alternation is limited.

Neverthelefs there is a term at which the phenomenon becomes irregular; and this is when the whole thicknefs of the metallic bafes and plates of glafs amounts to 16 or 20 millimetres (three quarters of an inch).

10. Though the above phenomena clearly prove that the metal does not act as a defence or cover in the production of thefe effects, the author proves it ftill more ftrongly in the following manner.

Gilt paper operates like a metal if the gold be outfide; otherwife not.

He applies to a pane of glafs gilt papers of the fame fize and figure, fome by their metallic faces, and others by the furfaces which are uncovered, all the other circumftances being the fame. Thofe papers only of which the metallic furfaces were expofed to the air produced the effects of metals, and the others produced no effect. This experiment repeated with pieces of glafs coated with tin foil inftead of gilt paper, produced the fame refults.

Infide metal at a diftance from the glafs caufes condenfation on the glafs, and not the metal.

11. If in the infide of a chamber, and parallel to one of the panes of glafs in a window, there be placed a difc of metal of fix or feven centimetres in diameter (two inches and a quarter) fo that it fhall be fupported only by the middle, and the face fhall be only a few millimetres (or twenty-fifths of an inch) diftant from the glafs, when the humidity comes on within there is none depofited on the difc, unlefs it be extremely near, but on the oppofite glafs it is depofited in much greater quantity than any where elfe.

12, If

12. If inſtead of this diſc of metal a diſc of glaſs be ſubſtituted, the humidity is not accumulated oppoſite this diſc any more than on the reſt of the glaſs.

Glaſs inſtead of metal has no effect.

13. The inverſe phenomenon takes place on the outer ſurface under like circumſtances.

Outſide, the reverſe.

14. If the metallic diſc (of ſection II.) be varniſhed on the ſide which faces the interior of the chamber, the phenomenon takes place in the ſame manner.

Varniſh on the face fartheſt from the glaſs has no effect

15. If the ſide next the window be varniſhed, or if both ſides be varniſhed, the phenomenon does not take place.

on the ſide next the glaſs, deſtroys the effect.

16. Theſe properties of the metals belong alſo to other conductors of electricity, but with ſome modifications according to their nature.

Other plates as well as metals.

17. If a plate of metal be ſubſtituted inſtead of a ſquare of glaſs, humidity is ſometimes produced, but always much leſs than upon glaſs.

Metal plate inſtead of the pane of glaſs.

18. If this plate of metal be thin and flat, and a diſc of glaſs be faſtened to it, the humidity is produced in preference, or more abundantly on the correſponding interior ſurface.

Pane of glaſs on a plate of metal.

19. All theſe phenomena take place in the ſame manner, or nearly ſo, when panes of glaſs fitted up with the metallic diſcs, or apparatus before deſcribed, are ſupported upon ſhort pegs in the middle of a field where the graſs has been mowed and clipped; in theſe circumſtances the upper ſurface of the glaſs repreſents the outſide, and the lower the inſide of the chamber.

The ſame phenomena in the open air, where the ground repreſents the chamber, and the upper ſpace the outer air.

20. The interior humidity may be excited provided the weather be rather cold, by means of a ſtove, or by ſprinkling water on a red hot body; and in the country during dry weather, the ſpace on which the glaſſes are to be expoſed may be lightly watered after ſun-ſet.

Artificial production of dew in a chamber; and u doors.

21. If different liquids ſuch as water, mercury, alcohol, oils, acids, &c or even ſmall leaden ſhot be put into glaſs veſſels equal and alike in all reſpects to the half, or two thirds of their height, and they be expoſed to the dew out of a window, the humidity will not be depoſited on the lower part of the veſſel, but only above the level of the ſubſtances contained, and at a diſtance which varies according to the nature of theſe ſubſtances, being greater for mercury than for water, greater for water than for oil, &c.

Other bodies different from metals.

22. In all theſe phenomena when the humidity is too abundant, the reſults become confuſed and uncertain.

The humidity muſt be moderate.

Difficult to shew whether the metals differ.

23. The author made several experiments to ascertain whether some of the metals might not be better adapted to the production of these phenomena than others. Having, for example, exposed to the dew plates of copper gilt and silvered, and plates of silver gilt only on the half of one of their faces, he observed nothing decisive. It only appeared to him, that in general the drops were formed larger on the white metals. But these comparative experiments are difficult to make on account of the readiness with which most of the metals become oxided in moist air; which then gives more or less of the property of glass.

Differences of temperature on the two sides.

24. In making these observations Cit. Prevost almost always kept account of the changes produced in the temperature, or the difference between the internal and external temperature. He endeavoured to procure thermometers with flat bulbs, sufficiently sensible to point out the difference of temperature of the two sides of the glass at the same instant, which he thinks would have afforded instructive results; but he has not yet succeeded. He only remarks that it is not necessary that the external temperature should, as is commonly thought, be more elevated than the internal, in order that humidity should be formed on the outside of the glass, but that the contrary often happens.

Farther researches.

25. These researches may be carried farther, for example, by making similar experiments in vacuo, or in other gases as well as air, and by employing other liquids instead of water.

26. In order to reduce the principal facts contained in his memoir to a small number of propositions, Cit. Prevost gives the name of the *armour of contact* to a metallic leaf applied or glued against the glass, and *armour of distance* to that which is some millimetres off from it. This being premised, the following are the general facts:

General statement of the facts.

Armour of metal on the warm face condenses moisture whether without or within;

I. When a partition or frame of glass, which separates two great masses of air at unequal temperatures, is armed in contact on its *warm face*.

A. If humidity be deposited on this face, it accumulates on the armour, insomuch that there is more there than elsewhere.

B. And if humidity be deposited on the opposite face, or the *cold face*, it accumulates on this face in the place corresponding with the armour, so that there is more there than elsewhere.

C. The

C. The ſame happens when the armour is at a diſtance; but then if the humidity be depoſited on the warm ſide, it is not on the armour, but it accumulates oppoſite to it upon the glaſs. on the glaſs if the armour be diſtant.

II. When the partition is armed in contact with the cold face. Armour on the cold face repels moiſture on both ſides,

A. If humidity be depoſited on this face, it is not on the armour.

B. And if humidity be depoſited on the oppoſite face, that is to ſay, the warm face, there is none on the correſpondent place of the armour.

C. The ſame happens when the armour is at a diſtance on the cold face. But then if the humidity be depoſited on this face, there is not only a want of it on the armour, but there is none on that ſurface of glaſs oppoſite the armour. if at a diſtance.

III. A. In order that the phenomena ſhould ceaſe to take place, it is ſufficient that the armours of contact, or the face of the armours of diſtance oppoſite the glaſs, ſhould be covered with glaſs or varniſh. Nothing remarkable then happens. Glaſs or varniſh prevents the effects of the armour.

B. If this glaſs or varniſh be again covered with metal, the phenomena again occur. Alterations of glaſs or metal.

C. They ceaſe if this metal be again covered with glaſs.

D. They begin, &c.

IV. One ſingle propoſition will include almoſt the whole of theſe facts.

When glaſs ſeparates two maſſes of air of unequal temperatures, the armour gives it (or ſeems to give it) the property of accumulating or repelling humidity, according as it is applied either on the warm *or* cold face, *and this influence extends through the glaſs and through other ſubſtances, to the diſtances of ſeveral centimetres.* General propoſition or ſtatement.

V. Or ſtill more ſimply.

Glaſs which ſeparates two maſſes of air of unequal temperatures, accumulates (ſeems to accumulate) or repells humidity, according as it is armed on the warm or cold face. Simple general law of thoſe facts.

EXPLANATION.

I imagined for a long time, ſays the author, that theſe phenomena depended on electricity, but they may be explained very naturally by the elective attractions which are exerciſed at a diſtance, and by the well known property of the metals of being good conductors of caloric.

BASES

BASES OF THE EXPLANATION.

General bafes for explaining them.

1. *The lefs the temperature of glafs is elevated, the more humidity it attracts from the air.*

2. *Metals attract it very little.*

3. *Glafs fenfibly exercifes its action on the humidity of air, at a diftance, and notwithftanding the interpofition of different bodies, fuch as plates of metal,* &c.

4. *Metals give to glafs, near which they are placed, the property of more fpeedily attracting caloric from hot air, and on the contrary, that of yielding it more fpeedily to cold air.*

N. B. When I fay that metals give glafs this property, I mean that *they act as if they gave it them;* which is evident by an examination of two thermometers, one of mercury the other of alcohol, which are plunged at the fame time in air either colder or warmer than that whofe temperature they indicate. The metallic thermometer arrives much fooner than the other to that of the new medium. Its glafs then, if colder, muft take up more fpeedily from the medium the caloric which it tranfmits to the metal, or, if hotter, it muft more fpeedily give out that of the metal.

Remarks.

The firft bafis has been long eftablifhed.

The fecond and third are proved by the 17th and 18th fections of the extract.

The laft is a neceffary confequence of the conducting property of metals.

This being premifed, it is eafy to comprehend that,

The laws applied to explain all the facts.

A. When the glafs is armed on its warm face (§ 27, 1ft, A. B. and C.) it yields its caloric to the cold air more fpeedily than that which is not armed (*bafis* 4), and confequently it attracts humidity more powerfully (*bafis* 1), whether directly on the glafs, or through the metal, or on the metal itfelf (*bafis* 3), if this be in contact; but if it be at the diftance of fome millimetres, the humidity not meeting the metal on its paffage, accumulates on the oppofite glafs in a greater quantity than elfewhere.

B. If the metal be applied on the cold fide (§ 27, II. A. and B.) the glafs moft heated does not attract fo much humidity (*bafes* 4 *and* 1), and it accumulates on the unarmed part of the pane.

C. If, in this cafe, the armour be covered with a plate of glafs, the plate cools more fpeedily than if the metal were not prefent;

ſent; but as the partition is more heated than if it were not preſent, there is no effect, and the totality of the double glaſs armed within, is in the ſame caſe with that unarmed; it therefore accumulates neither more nor leſs humidity.

A ſecond armour on the plate of glaſs will cauſe the phenomena to re-appear; a ſecond plate of glaſs on this new armour will again make it diſappear, &c. For as long as the ſymmetrical glaſs ſhall have the interior armours, the cauſes of heat and cold will be found in equilibrio; but an additional armour will neceſſarily deſtroy the balance, and the heated glaſs will not attract humidity; which explains the facts of § 27. III. A. B. C. D. &c.

D. If the glaſs be armed on both ſides (§ 7), as it would not then be expoſed to the air, either on the cold or warm ſide, it ſeems that it ought to attract as much humidity on the armours as on the reſt of the partition. But though the glaſs exerciſes its action through the metal, this is nevertheleſs an obſtacle which diminiſhes its force; humidity in this caſe will not then be ſo ſtrongly attracted by the doubly armed glaſs as by the part perfectly unarmed, &c.

CONCLUSION.

Concluſion.

Theſe obſervations are not only intereſting, but they appear to eſtabliſh an important point in philoſophy; namely, that *glaſs exerciſes its attraction for the humidity (which has a tendency to be depoſited from the air) through metals.* See likewiſe §§ 19 and 26 for the generalization of this propoſition.

EXPLANATION OF THE FIGURES.

Explanation of the figures.

Fig. 1, Plate XV. relates to Art. 4 of the abſtract. A is the interior rectangle more deeply ſhaded than the reſt of the ſquare, which ſignifies that it is more moiſtened. B is the place correſponding to the exterior rectangle, with its ſurrounding part repreſented white, becauſe dry.

Fig. 2 relates to §§ 7 and 8.

Fig. 3, 4, 5, 6, 7, relate to Art. 7 and 9. I c ſignifies *interior ſide hot*; E f, *exterior ſide cold.* The lines more deeply ſhaded repreſent the ſection of the metallic leaves; the white lines repreſent the ſection of the glaſs, and the dotted ſhades repreſent drops of dew or moiſture.

Fig. 8 relates to Art. 11.

SCIENTIFIC

SCIENTIFIC NEWS, ACCOUNT OF BOOKS, &c.

Abstract of the late Experiments of Professor ALDINI *on Galvanism.*

Galvanic experiments of Aldini.

M. ALDINI, Professor at the Institute of Bologna, and nephew of the celebrated Galvani, after having made his experiments at the National Institute of France, has visited London, and given an accurate account of his experiments and discoveries to the Royal Society, before whom the same was read on the 25th last. I have the pleasure to communicate some of the principal facts which he has had the goodness to communicate to me, which appear calculated to throw much light on some of the most difficult phenomena of nature.

The theory that electricity is generated in animals,

Various philosophers have considered the metals as not absolutely necessary for the production of galvanism, and Mr. Davy has proved it in the pile: It has also been indistinctly apprehended or conjectured in the way of theory, that the galvanic or electric matter was excited, collected, or generated in the bodies of animals, where it was considered as the great cause or instrument of muscular motion, sensation, and other effects highly interesting, but very little understood. Professor Aldini has the distinguished merit of having placed this proposition in the rank of established truths. He has succeeded in exciting muscular contractions by the simple application of the nerves to the muscles of a prepared frog, without the least suspicion of any stimulus arising from contact. He has also given motion to the limbs of a small cold blooded animal by the galvanic energy of an animal with warm blood; an experiment never before imagined. He takes the head of an ox recently cut off, and applying the finger of one hand wetted with salt water to the spinal marrow, he holds in the other hand the muscle of a frog prepared (that is by dissection) in such a direction that its crural nerves shall touch the cervical muscles on the tongue of the ox. Every time of this contact strong contractions are produced in the frog. If a chain of several persons be formed holding hands, the same effect takes place; but the contacts do not produce any effect if the chain of connection be broken or interrupted. Here then we have the most decided substitution of the organized animal system in the place of

verified by Aldini;

by substituting part of an animal instead of the metallic pile.

The head of an ox convulses part of a frog.

of the metallic pile: it is an animal pile; and the production of the galvanic fluid, or electricity, by the direct or independent energy of life in animals, can no longer be doubled.

The Professor has lately repeated the series of these experiments at Oxford, and shewed in the presence of Doctors Pegg and Bancroft, that the nerves of a prepared frog, disposed in the manner here stated, approach very sensibly to the muscles of the warm-blooded animal, and exhibit a real attraction hitherto unknown in natural philosophy and physiology. He invites philosophers to vary and repeat this phenomenon, which has been confirmed by different philosophers, particularly by the celebrated Felice Fontana of Florence. Galvanism by these facts, is shewn to be animal electricity, not merely passive, but most probably performing the most important functions in the animal economy. This power appears not to be confined in its operation to the motion of the muscles, but also appears to be of importance in the secretions. Aldini has given strength to this conjecture, by subjecting the urine to the power of the artificial galvanic stream, and he found it productive of a separation of the principal combined parts of that fluid, which were considered as of much importance by the celebrated Professors Senebier and Jurine, of Geneva.

These experiments repeated at Oxford.

Animal attraction, verified.

Galvanism operates in the secretions.

A very ample series of experiments were made by Professor A. which shew the eminent and superior power of galvanism beyond any other stimulant in nature. In the months of January and February last, he had the courage to apply it at Bologna to the bodies of various criminals who had suffered death at that place, and by means of the pile he excited the remaining vital forces in a most astonishing manner. This stimulus produced the most horrible contortions and grimaces by the motions of the muscles of the head and face; and an hour and a quarter after death, the arm of one of the decollated bodies was elevated eight inches from the table on which it was supported, and this even when a considerable weight was placed in the hand. These experiments have since been confirmed in various parts of Italy, particularly at Turin, by Professors Giulio, Vassali, and Rossi.

Astonishing effects on human subjects.

Contortions of the muscles, &c. of the face: the arm lifted up.

These are not experiments of pure curiosity, but offer very encouraging prospects for the benefit of mankind, in disorders of the head, and in apoplexies. Professor Aldini means to apply

These experiments promise great benefits in the cure of disorders.

part

part of his time in London, in communicating thefe important fubjects of information to phyficians, as he has already done in Paris, in which place he made fome applications of his difcoveries, chiefly at the Hofpital de Salpetriere, in company with Doctor Pinel*. The application of galvanifm in melancholic infanity is abfolutely new, and of great intereft. He perfectly cured two patients at Bologna of this diforder; and on that account he is more defirous of recommending the trial in an affliction fo diftreffing, againft which the prefent fyftem of phyfic has fo little to offer.

Infanity cured.

Apoplexy.

Recovery of the drowned.

It appears to be equally promifing in cafes of apoplexy. Aldini thinks it may be highly ufeful in recovering drowned perfons, and on that account he is defirous of communicating with the Society eftablifhed at London for the recovery of thefe unfortunate individuals. An experiment lately made at Paris adds much to his expectations. At the Hofpital de la Charité he fhewed the pupils, that galvanifm applied to the trunk of a dog, to the fpinal marrow, and the inteftines, caufed the lungs to act in fuch a manner that the air that iffued from the afpera arteria twice in fucceffion, extinguifhed a large candle placed oppofite. Now, as in moft cafes little more is required in drowned fubjects, than to fet the organs of refpiration into action, it is to be prefumed that the action of galvanifm may be of the higheft utility in thefe cafes. Many precautions are neceffary to be ufed in the adminiftration of this powerful remedy in lunacy or apoplexy, which will be detailed in a large work which Profeffor Aldini intends to publifh in this country before his return to Italy. In the mean time the reader will doubtlefs receive fatisfaction from this fhort notice he has enabled me to give of his labours on a fubject which promifes greatly to extend the limits of natural fcience, and may be reafonably expected to add to the powers which man is enabled to exert for his own benefit over the numerous beings around him.

Refpiration ftrongly excited by galvanifm.

Precautions.

Profeffor Aldini intends to publifh a large work.

* Whofe *Treatife fur la Manie* is well known in this country.

" *Examen,*

"*Examen, &c." Chemical Examination of a New Gas, composed of Hidrogen, Carbon, and Phosphorus. By* J. B. TROMMSDORFF.

Mr. Trommsdorff obtained this gas during the decomposition of phosphoric acid by ignited charcoal. In its common state it is mixed with carbonic acid, which may be separated from it by agitation in lime water. A new gas by Trommsdorff.

The new gas is nearly of the same specific gravity as common air; it is insoluble in water, and undergoes no change when mixed with oxigen, at common temperatures; but it detonates with that aëriform fluid by the action of heat. It is possessed of no agency upon the solutions of metallic oxides which are not reducible by heat, but it decomposes the fluid saline compounds containing gold, silver, or mercury. During its combustion with oxigen, water, phosphoric acid, and carbonic acid are formed, and hence Mr. Trommsdorff is inclined to conclude that it is a triple compound of phosphorus, hidrogen, and carbon; and he proposes to call it by a name, which may be translated by the term of phosphorated carbonated hidrogen gas.

A part only of the memoir from which this account is taken is as yet published. Concerning the action of the new gas upon metallic solutions, and other phenomena presented by it, the learned author promises to enter upon some additional details. Without wishing to anticipate any of his reasoning upon the subject; with the simple hope of throwing out a hint for future discussions, we shall venture a general observation or two in relation to it.

If hidrogen exists in the gas, it must apparently arise from the decomposition of water contained by the concrete acid or the charcoal; for, as would appear probable from the experiments of Desormes and Clement, well burnt charcoal contains no ascertainable quantity of combined hidrogen. By separately igniting the phosphoric acid and the charcoal, before they were made to act on each other, the water contained by them would be driven off; and, under such circumstances, it would be curious to ascertain if the gas of Mr. Trommsdorff would be produced.

As

As phosphorus had a very strong affinity for oxigen, we should be disposed, *a priori*, to conclude, in reasoning upon the facts lately discovered concerning the gaseous oxide of carbon, that this substance would be formed and evolved with the carbonic acid in the process of the decomposition of phosphoric acid by charcoal. Is it not possible that the new gas may be a mixture of a triple compound of carbon, phosphorus, and oxigen, with carbonated hidrogen produced from the decomposition of water united to the primary ingredients? There is nothing in the experiments detailed in the memoir which militates against this supposition, and it might be submitted to the proof of a new experiment at the same time with the theory of the author.

D.

Notice concerning certain peculiar Habits of the Shark and Pilot Fish, by GEOFFROY, *Professor at the Museum of Natural History.*

Interesting narrative of shark and pilot fish.

IT is asserted, that the shark has brought under his command, a very small fish of the species of the gadus, that this latter fish precedes its master in its progress, that it points out those parts of the sea which most abound in fish, and the method of obtaining the prey of which he is most greedy; and that the shark, notwithstanding its extreme voracity, in acknowledgment for these signal services, lives on good terms with this useful companion. Naturalists, who are always guarded against the exaggerations of travellers, and not being able to account for such an association, have doubtfully rejected these facts. It will be seen that they are wrong in so doing: the observations which I have made are accompanied by circumstances, which perhaps were never presented to any one but myself in so detailed a manner.

The 6th Prairial in the year 6, I was on board the frigate *Alceste* between Cape Bon and the Island of Malta. The sea was calm: the passengers were tired with its long duration, when their attention was arrested by a shark which they perceived approaching the vessel. It was preceded by pilot fish, who were nearly at the same distance from one another as they were

were from the ſhark: the pilot fiſh approached the poop of the veſſel, they examined it twice from one end to the other, and being ſatisfied that there was nothing there to anſwer their purpoſe, they continued the courſe they had formerly held. During theſe different movements, the ſhark never loſt ſight of them, or rather he followed them ſo exactly, that it ſeemed as if he had been drawn along. Interesting narrative of ſhark and pilot fiſh.

As ſoon as they were diſcovered, a ſailor immediately prepared a large fiſh hook, which he baited with pork; but the ſhark and his companions, by the time the fiſher had got ready, were at the diſtance of 20 or 25 metres; he nevertheleſs threw his bait into the ſea. The noiſe occaſioned by the fall was heard at a diſtance. The fiſh were aſtoniſhed and ſtopped their courſe; the pilots immediately turned, and prepared again to examine the poop of the veſſel. The ſhark, during their abſence, amuſed himſelf in a thouſand different ways at the ſurface of the water: he turned on his back, then on his belly, dived into the ſea, but always appeared again in the ſame place. The pilots having arrived at the poop of the *Alceſte*, paſſed near the pork, and as ſoon as they perceived it, they returned to the ſhark with greater ſwiftneſs than when they left him. When they had reached him, he began his courſe: the pilots ſwimming one on his right and the other on his left, exerted themſelves to keep before him; they with difficulty did ſo, and returned a ſecond time to the poop of the veſſel; the ſhark followed them, and owing to the ſagacity of his companions, perceived the prey that was deſtined for him.

It has been ſaid that the ſhark has a very powerful ſmell: I paid much attention to what paſſed when he was near the pork: it ſeemed to me that he did not perceive it till the very moment that his guides, as it were, pointed it out to him; he did not till then ſwim with greater ſwiftneſs, or rather make a ſpring to ſeize it. He firſt ſeized a portion without being hooked; but at the ſecond attempt the hook pierced the left lip: he was hoiſted on board.

At the expiration of two hours, during which I was occupied in the anatomy of this ſqualus, I began to regret that I had not more nearly examined this ſpecies, which ſo willingly conſecrates itſelf to the ſervice of the ſhark: I was aſſured that

that I might eaſily do it, as it was certain that they had not quitted the neighbourhood of the veſſel, and ſome moments after they pointed one out to me, which I perceived was of the ſpecies of pilot fiſh of the ſailors, and of the *gaſteroſteus ductor* of naturaliſts.

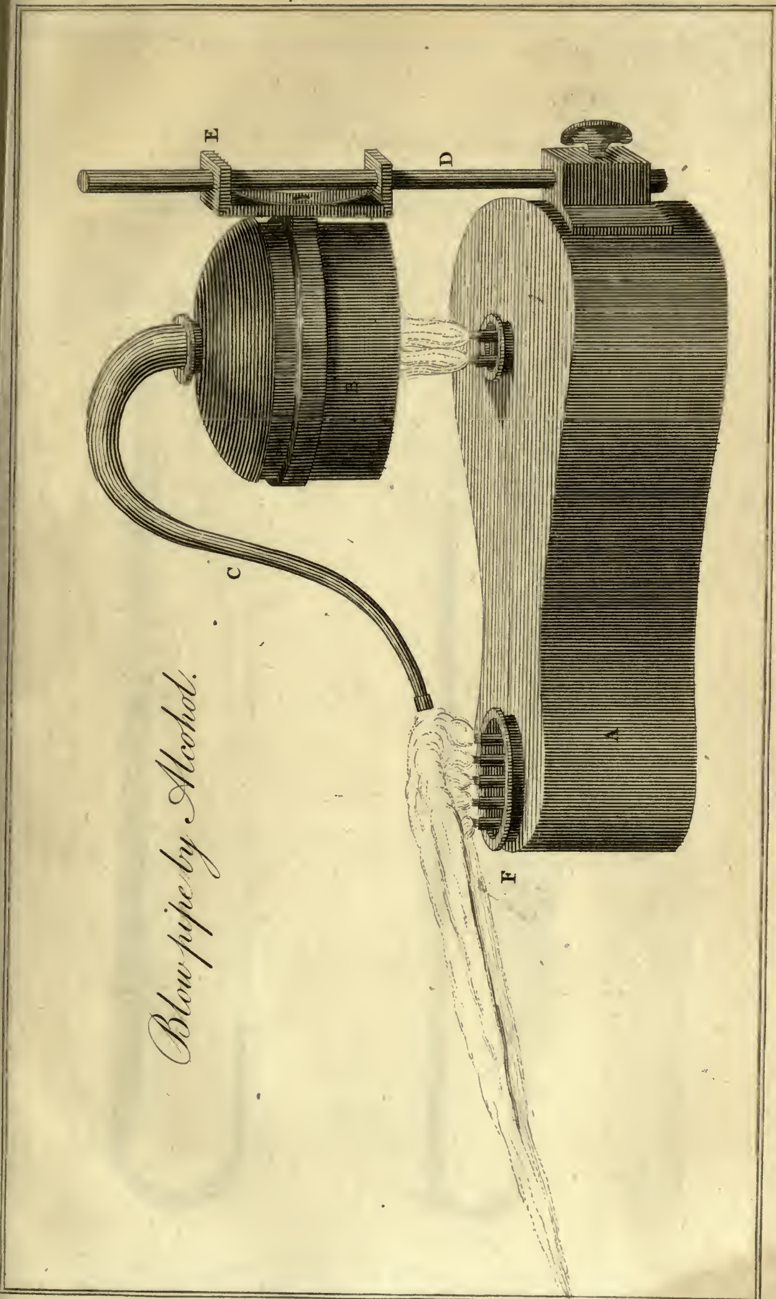
It would doubtleſs be curious to examine what intereſts can induce animals ſo different in their organization, their ſize and habits to form ſuch an intercourſe. Does the pilot fiſh feed on the dung of the ſhark as Cit. Boſc has ſuppoſed; or does it impoſe on itſelf the painful duties of ſervitude, in order to find protection and ſafety in the company of ſo voracious a ſpecies.

Notice of a New Metal, by Profeſſor Proust.

New metal Silemes.

Profeſſor Prouſt, in a letter to Dr. Delametherie, promiſes to ſend an account of a new metal he has lately diſcovered in an ore from Hungary. He is not yet acquainted with its metallic aſpect, he is apprehenſive it will not be eaſily reduced, from its retaining oxigen with conſiderable avidity, and like many other metals, it is ſuſceptible of two degrees of oxidation: the ſolutions of its oxide at maximum is yellow, green at the minimum, when in theſe two ſtates it colours glaſs; laſtly, it may be ranked in tne claſs of thoſe metals which retain oxigen againſt ſulphurated hidrogen. He has purified it by the ſame means he employed with nickel, cobalt, iron, manganeſe.

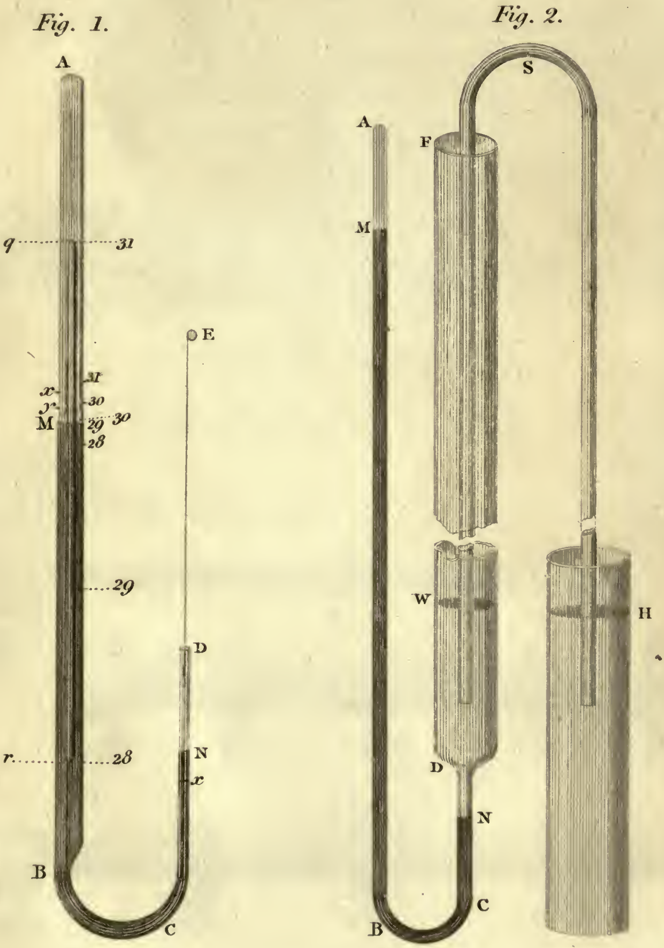
J. de Phyſ. Vendemaire, an 11.



Mutlow Sc. Russell Co.

Barometers.

by the Rev. James Wilson, A. M.

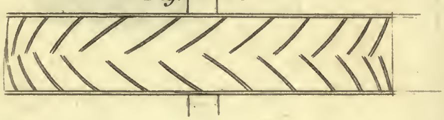


Mutlow sc. Russell Cot.

Mr. Besant's Water Wheel.

Fig. 1.

Fig. 2.



Mr. R. Phillips's Tubes for driving Bolts into Ships

Fig. 3.

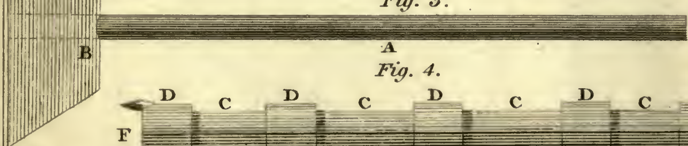
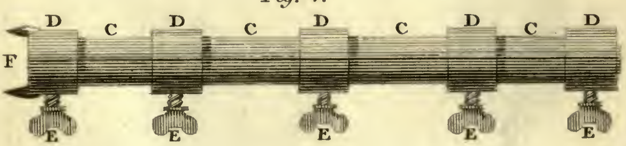


Fig. 4.



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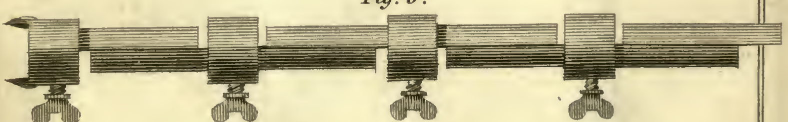


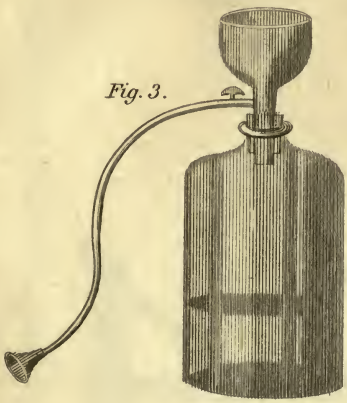
Fig. 6.

Mutlow Sc. Russell Co.

Mr. Read's pneumatic Apparatus.

Hydrometer for directly exhibiting Specific Gravities.

Fig. 3.



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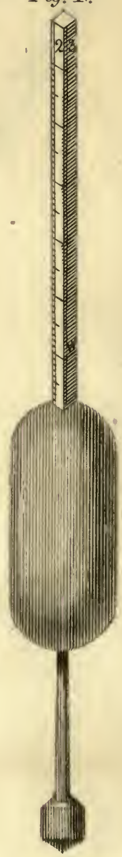

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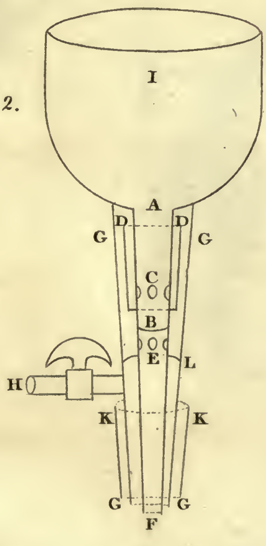
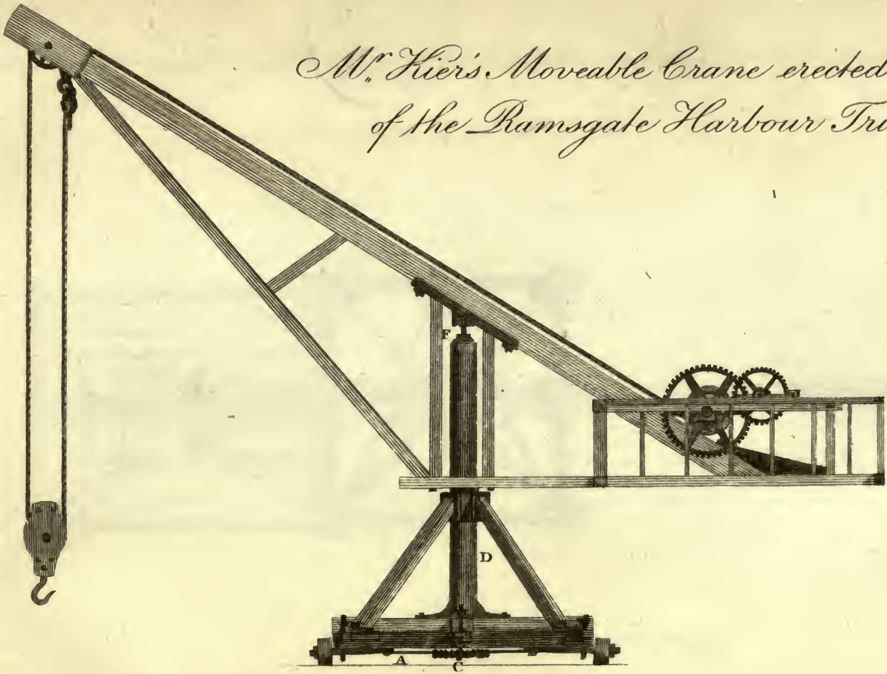


Mutlow Sc. Russell Ct.

Mr. Kier's Moveable Crane erected by order of the Ramsgate Harbour Trust 1802.



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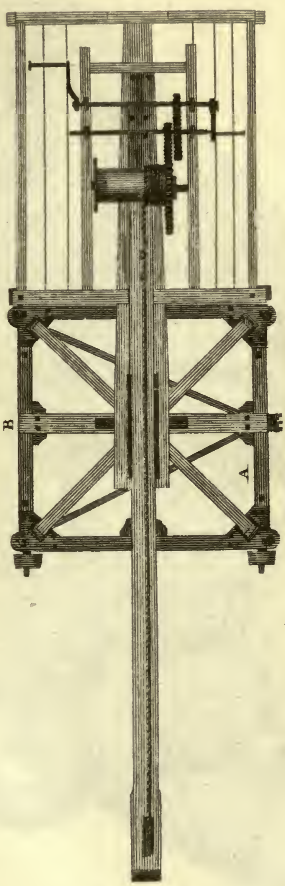

Plan of the Crane.

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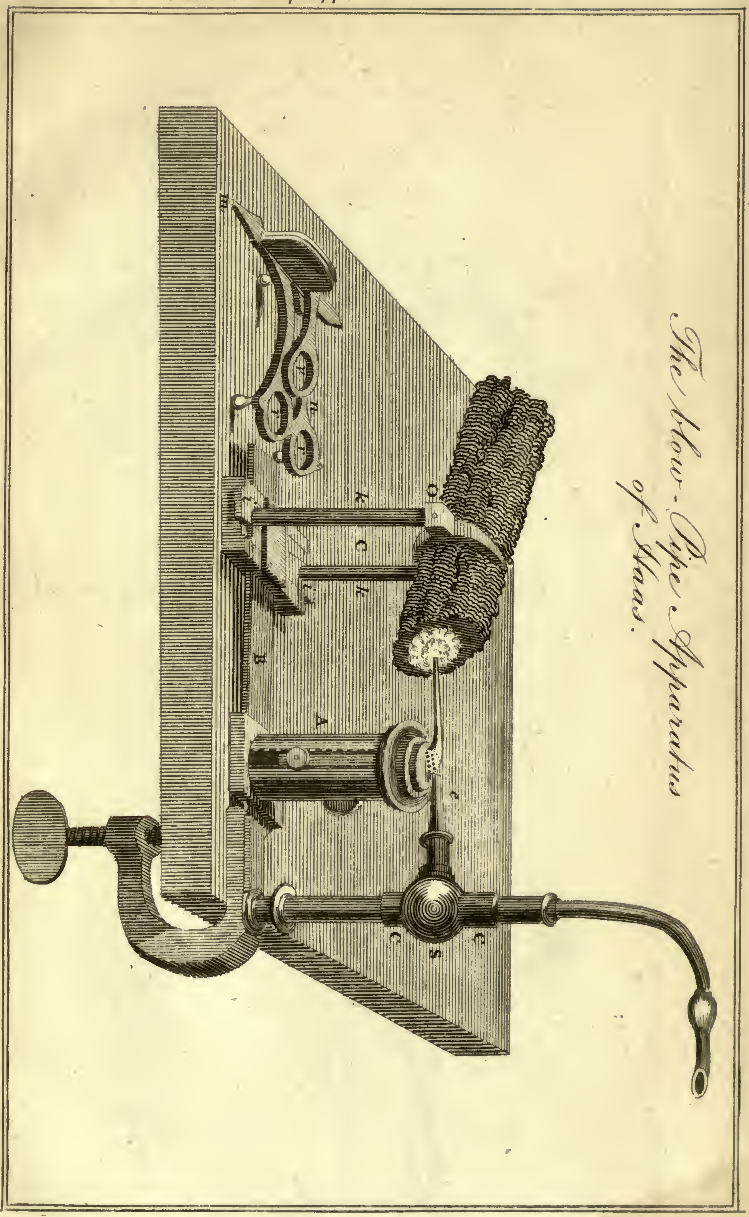


The Blow-Pipe Apparatus of Haas.

Mutlow Sc. Russell Ct.

B. *Basalt on an uneven bottom.*

Fig. 1.

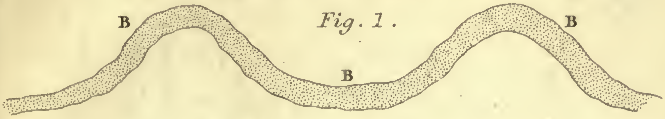


Fig. 2. B. Bed of Basalt broken by Water.

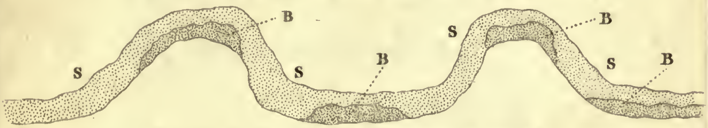
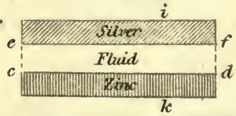


Fig. 2. S. Bed of Sandstone deposited on the broken bed of Basaltes.

Fig. 3.

Elements of the Galvanic Pile.



Distinguishers of Galvanism & Electricity.

Fig. 4.

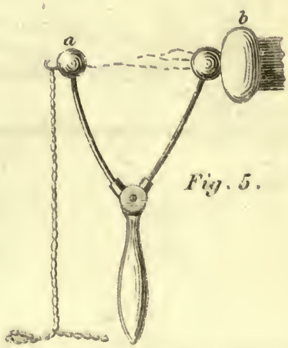


Fig. 5.

Mutlow Sc. Russell Ct.

Expansions of the Gases by Heat.

Fig. 1.

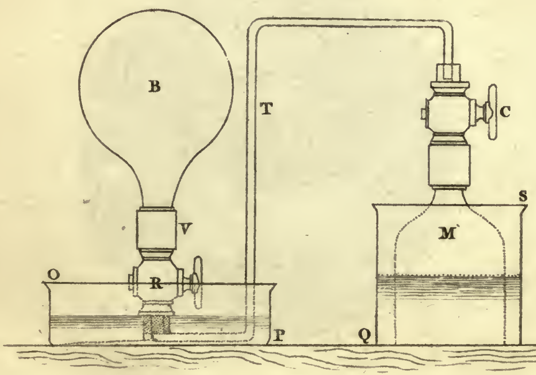
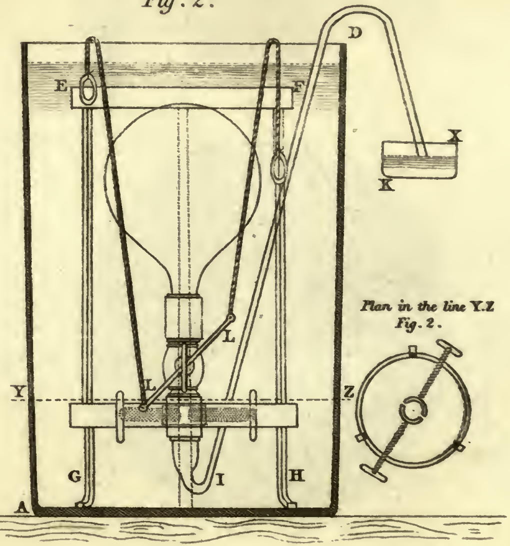


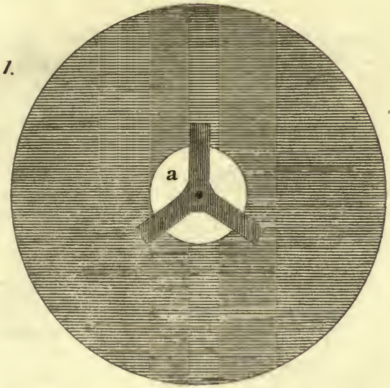
Fig. 2.



Plan in the line Y.Z
Fig. 2.

East Indian Hand Mill.

Fig. 1.



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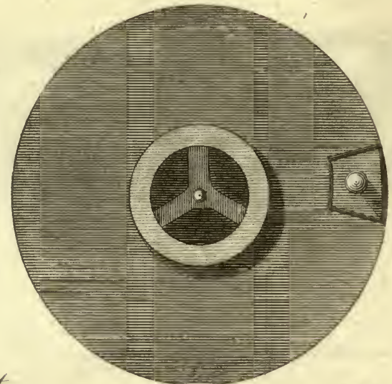


Scale 1 inch one foot

Fig. 4.

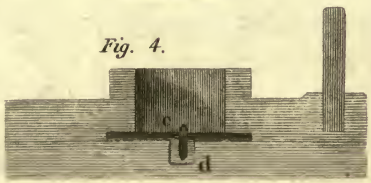
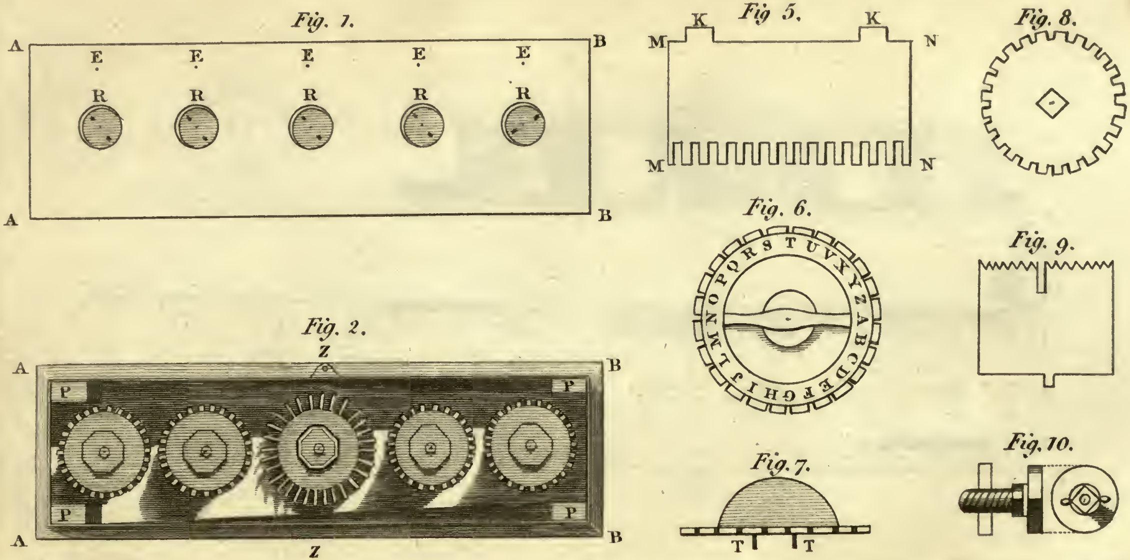


Fig. 3.

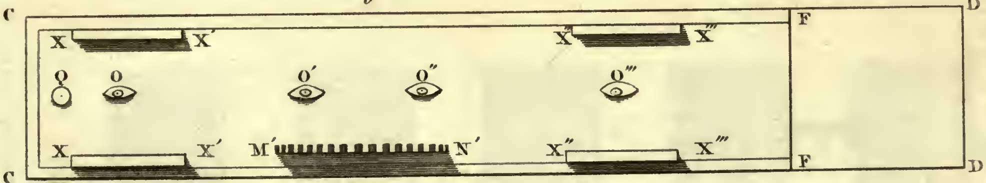
Mutlow Sc. Rußell Ct.

Secret Lock of Berard.



Mutlow sc. Rayfell &c.

Fig. 3.



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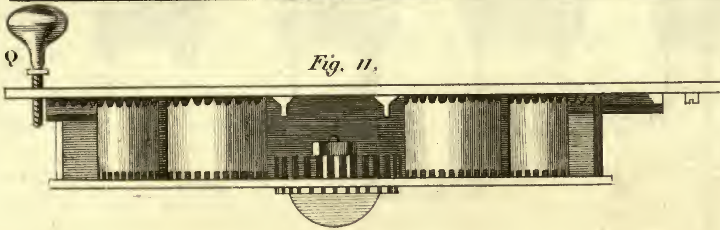

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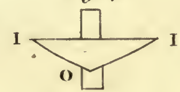
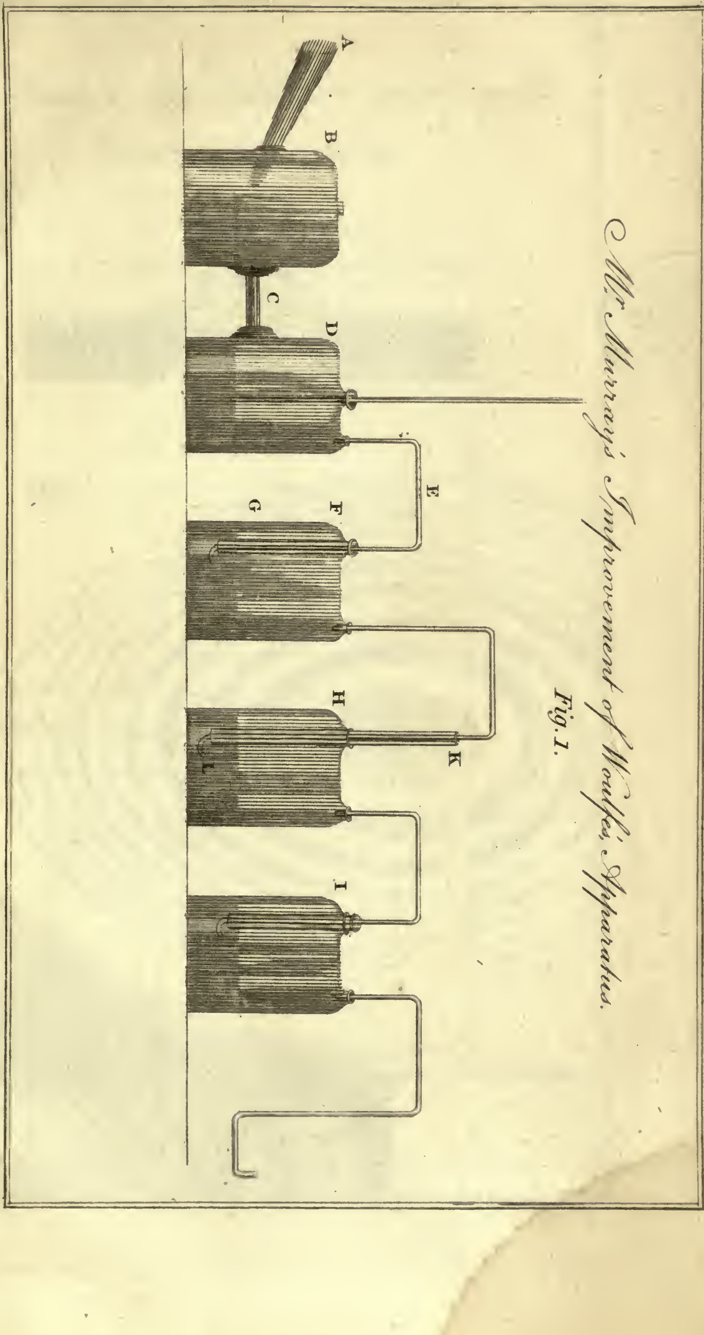


Mutlow sc. Russell Cr.



Mutlow sc. Russell Ct.

D.r Young's Apparatus for exhibiting the Colours of thin plates.

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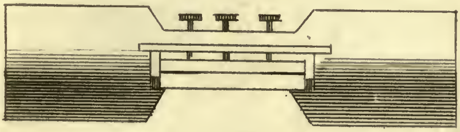
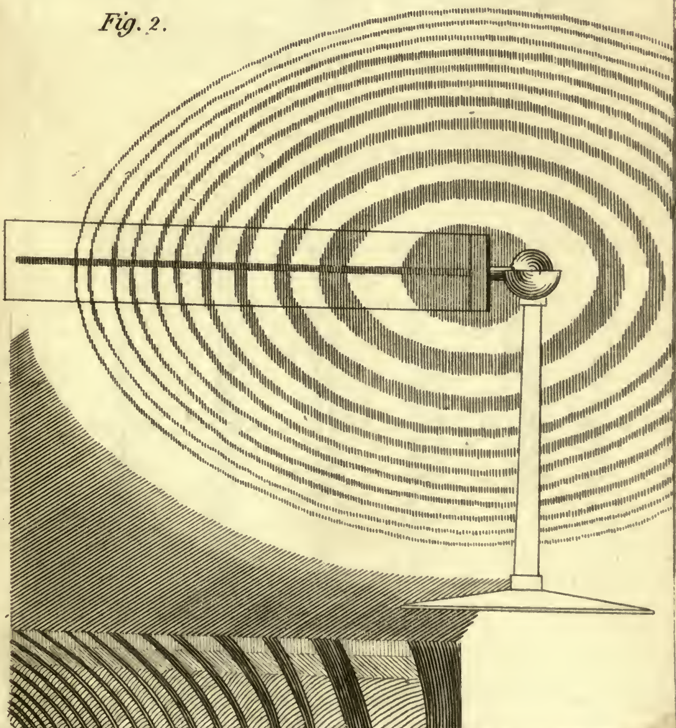


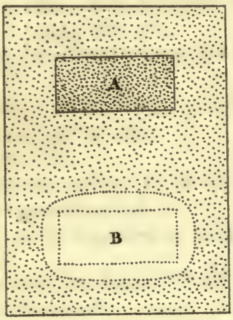
Fig. 2.



Mutlow sc. Rupell Co.t

Prevost on the Phenomena of Dew.

Fig. 1.



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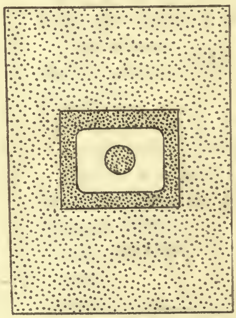
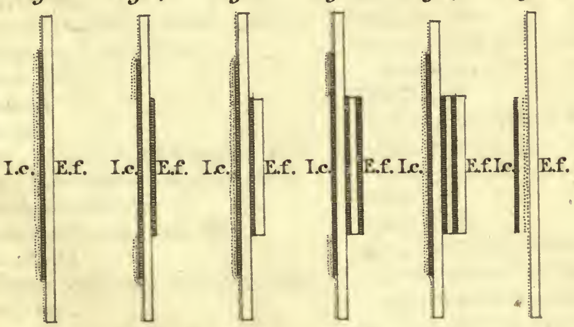


Fig. 3. *Fig. 4.* *Fig. 5.* *Fig. 6.* *Fig. 7.* *Fig. 8.*



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